

DISCUSSIONS ON Child Development

The First Meeting of the
World Health Organization
Study Group on the
Psychobiological Development
of the Child
Geneva 1953

EDITORS

J. M. TANNER AND BÄRBEL INHELDER

PREFACE BY PROFESSOR G. R. HARGREAVES

In 1953 the World Health Organization brought together some dozen experts to discuss the influences of biological, psychological, and cultural factors in the development through childhood of the adult personality. This group, of international composition, represented a wide range of scientific disciplines: for example, from ethology, Konrad Lorenz; from anthropology, Margaret Mead; from psychology, Jean Piaget, Bärbel Inhelder, and René Zazzo; from psychoanalysis, John Bowlby; from electrophysiology, W. Grey Walter, A. Rémond, Marcel Monnier, and K. A. Melin; from human biology, J. M. Tanner; and from research promotion, R. R. Struthers.

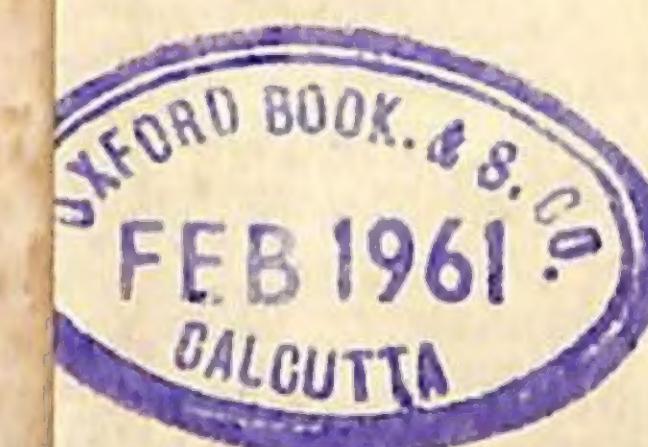
The group, with Dr. G. R. Hargreaves, then Chief of the Mental Health Section of W.H.O., as Secretary, has met each year under the chairmanship of Dr. Frank Fremont-Smith, Medical Director of the Josiah Macy Jr. Foundation of New York. There have so far been three meetings of the group and a fourth is shortly to take place.

At each meeting two or three guests are invited to participate. These included, for the first meeting, Dr. Carothers, Dr. Krapf, and Dr. Charles Odier; for the second, Professor Howard Liddell of Cornell University, Dr. Bindra of McGill University, Professor Whiting of Harvard University, and Dr. Buckle of W.H.O.

The discussions, lasting about a week, are recorded in the form of a verbal transcript. The present volume is the edited version of the transcript of the discussions at the first meeting; it contains an introductory chapter wherein each member of the group tells the others a little about his scientific background, why he is interested in child development and its problems, and in what sphere he feels able to contribute to the joint discussions. The following chapters include presentations and discussions on the physical development of the child, on Piaget's criteria of the stages of mental development, on Wallon's descriptions of motor development in early childhood, on the electroencephalographic development of children, and on the light that ethology, psychoanalysis, and cross-cultural studies can throw on the fundamental problems in the development of human personality.

It is unlikely that so distinguished a group of people from so many disciplines and from so many countries has ever before been brought together for informal discussion in the field of human biology. Presentations of material by each member have been kept to a minimum, and most of the text is occupied with very free and wide-ranging discussion. The verbatim transcript has by careful editing been condensed to make about one third of its original length and to present the material in a more readable form, but the spontaneity of informal discussion in a small group has been retained. This book and the volumes that will follow it (Volume II immediately) will be found to be of the greatest interest and value to all those concerned with the genesis of the adult human personality, whether their prime concern is with physical or psychological or sociological factors.

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DISCUSSIONS ON
CHILD DEVELOPMENT

VOLUME ONE

DISCUSSIONS ON Child Development

A Consideration of the Biological, Psychological, and
Cultural Approaches to the Understanding
of Human Development and Behaviour

EDITORS

J. M. TANNER

M.D., PH.D., D.P.M.

Lecturer, Institute of Child Health, University of London

BÄRBEL INHELDER

Professor of Child Psychology, University of Geneva

VOLUME ONE

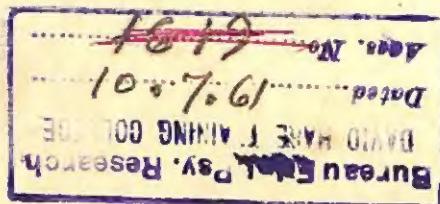
*The Proceedings of the First Meeting of the
World Health Organization Study Group
on the Psychobiological Development of the Child
Geneva 1953*



TAVISTOCK PUBLICATIONS LTD

*First published in 1956
by Tavistock Publications Limited
2 Beaumont Street, London, W.1
and printed in Great Britain
in 10pt. Times Roman by
The Pitman Press, Bath*

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MEMBERS OF STUDY GROUP

DR. JOHN BOWLBY Director, Children's Department Tavistock Clinic, London	<i>Psychoanalysis</i>
DR. FRANK FREMONT-SMITH Chairman Josiah Macy, Jr. Foundation, New York	<i>Research Promotion</i>
DR. G. R. HARGREAVES Formerly Chief, Mental Health Section World Health Organization Professor of Psychiatry, Leeds University	<i>Psychiatry</i>
MLLE. BÄRBEL INHELDER Professeur de Psychologie de l'Enfant Institut des Sciences de l'Education de l'Université de Genève	<i>Psychology</i>
DR. KONRAD Z. LORENZ Forschungsstelle für Verhaltensphysiologie des Max-Planck Institutes für Meeresbiologie Buldern über Dulmen, West Germany	<i>Ethology</i>
DR. MARGARET MEAD Associate Director Dept. of Anthropology American Museum of Natural History, New York	<i>Cultural Anthropology</i>
DR. K. A. MELIN Director, Clinic for Convulsive Disorders, Stora Sköndal, Stockholm	<i>Electrophysiology</i>
DR. MARCEL MONNIER Chargé de Cours de Neurophysiologie appliquée Université de Genève	<i>Electrophysiology</i>
PROFESSOR JEAN PIAGET Professeur de Psychologie à la Sorbonne et à l'Université de Genève	<i>Psychology</i>
DR. A. RÉMOND Chargé de Recherches, Centre National de la Recherche Scientifique, Paris	<i>Electrophysiology</i>

DR. R. R. STRUTHERS

Formerly, Associate Director
Rockefeller Foundation, Paris

Research Promotion

DR. J. M. TANNER

Formerly Senior Lecturer,
Sherrington School of Physiology,
St. Thomas's Hospital
Lecturer, Institute of Child Health
University of London

Human Biology

DR. W. GREY WALTER

Director of Research
Burden Neurological Institute, Bristol

Electrophysiology

RENÉ ZAZZO

Directeur du Laboratoire de Psycho-
biologie de l'Enfant
Institut des Hautes Etudes, Paris

Psychology

GUESTS

DR. J. C. CAROTHERS *Psychiatry*

Psychiatrist, St. James' Hospital
Portsmouth, England

DR. E. E. KRAPF *Psychiatry*

Associate Professor of Psychiatry
University of Buenos Aires

DR. CHARLES ODIER *Psychoanalysis*

Château de Vernand, Lausanne

PREFACE

This volume, and others which will follow it, give an account of an activity of the World Health Organization—the Research Study Group on the Psychobiological Development of the Child.

The popular view of the World Health Organization associates it chiefly with practical activities concerned with the application of existing knowledge through public health services and as far as the great majority of the Organization's activities have been concerned this view is accurate. More than half the Organization's funds have from its creation in 1948 been devoted to such programmes as the control of communicable diseases.

But the Organization differs from the International Health bodies which preceded it in having placed upon it a specific obligation to 'foster activities in the field of mental health, especially those affecting the harmony of human relations'. In this field the mass programmes appropriate to such problems as the control of malaria are not applicable. In the mental health field there are no equivalents to D.D.T. and penicillin, and aetiological knowledge is scanty.

Although the Organization has devoted most of its energies, and its funds, to the preventive application of knowledge gained by research in national institutions, it is also obligated by its constitution to promote and conduct research and it was as a contribution to the promotion of research that the Research Study Group was formed. It was not an isolated activity but was part of one of the natural trends of development of the mental health programme which began in 1949.

The World Health Organization makes considerable use of Expert Committees (small groups of distinguished workers in a given field drawn from different countries) to advise it on the development of its technical policy. Such an Expert Committee on Mental Health was convened in 1949. This Committee recommended that it was desirable that the Mental Health Programme should concentrate especially on the psychiatry of childhood. It emphasized the fact that others apart from psychiatrists must be called on to contribute in this field and mentioned specifically the anthropologist, the sociologist, and the social and developmental psychologist. In its report the Committee urged that W.H.O. should also 'actively encourage research which sets out to fill gaps in fundamental knowledge' in the mental health field, and in this connection it specifically mentioned 'Research into the biological, psychological, and cultural determinants of personality structure'. It is evident, however, that since the funds available for the mental health programme have been small—less than two per cent of the Organization's budget—the Organization could not itself finance research on any appreciable scale. The Organization's contribution has, therefore, been directed toward the co-ordinating, surveying, and stimulating of research.

Two such W.H.O. surveys on different aspects of the Mental Health of

Childhood were widely read. The first of these was 'The Psychiatric Aspects of Juvenile Delinquency' by the late Dr. Lucien Bovet, and the second by Dr. John Bowlby on 'Maternal Care and Mental Health' surveyed existing research on the psychological effects of the continued separation of infants from their mothers, or mother substitutes.

Both these surveys posed questions which gave rise to the thought of a multiprofessional discussion group on child development problems. The thought was crystallized into a concrete plan by the stimulus of the Oxford Conference of the Mental Health Research Fund on 'Prospects in Psychiatric Research' held in 1952, and the model on which the plan was based was the informal conferences of the Josiah Macy Junior Foundation.

The aim of the Group was to bring together once a year for four or five years, during a period of a week, a small number of internationally eminent workers in the different disciplines which study different aspects of the psychobiological development of the child.

The aim of the meeting was not the reading of papers, the passing of resolutions, or the issuing of a report, but the provision of an opportunity for mutual understanding to develop between workers in different disciplines, and on the basis of that understanding the attempt to relate the findings of one discipline to those of another and the hope that new research, and particularly joint research, might be undertaken. The Organization was fortunate in obtaining, as chairman of the Group, Dr. Frank Fremont-Smith, of the Josiah Macy Junior Foundation. His personal qualities, and his great experience of conferences of this type, created in this international group (despite the problems of simultaneous interpretation) the atmosphere and enthusiasm which made the success of the group possible.

It was not originally intended that the proceedings of the meetings should be published; but the mimeographed transcript which was produced for the benefit of the members evoked such interest in those outside the group who read it that many requests for copies began to be received. Hence the decision to publish this series. To make the publication possible considerable editing has been necessary since the original transcript had to be reduced by about two-thirds. The Group is much indebted to one of its members, Dr. J. M. Tanner, for carrying out this task so skilfully.

Finally, as the Chief of the Mental Health Section of the World Health Organization at the time these meetings were convened, I should like to express my personal appreciation to Dr. Brock Chisholm and Dr. Marcolino Candau, the first two Directors General of the Organization, and to Dr. Norman Begg, Regional Director for Europe, for their support of a venture which, although not connected with the day-to-day practice of current public health work, may yet through some of its many repercussions have its significance for public health workers of future generations.

G. R. HARGREAVES

Leeds University

Lately Chief, Mental Health Section, World Health Organization

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Introduction

FREMONT-SMITH (Chairman):

Je suis tout-à fait content d'être ici avec vous, mais malheureusement je ne peux pas parler le français, and so I will go on, not speaking in good English, but in the patois of the U.S.A. What I now have to say is intended to be introductory, and to tell you what we would like to have as the mood of the conference. I have been co-opted to this job because it was hoped that we could use at least some of the Macy Foundation conference methods in this meeting, and so first I should say a little about them. The Macy Foundation is a charitable body that makes grants for research. In the course of doing this the Foundation directors and officers became disturbed by the narrowness of the approach of the investigators who asked us for help. Their projects were drawn up from a unilateral point of view, and it seemed more and more that, to make advances in practically any problem in science, one needed the participation of several disciplines. We found that the investigator was frequently ignorant of the contribution already made or potentially to be made by another discipline, and too often was uninterested. The general position that we held was that nature is all of one piece and that the departments of universities, the specialities which had grown up for good and necessary reasons, tended artificially to divide that nature and to set up barriers which prevented communication between the different disciplines. Our conference programme arose as an effort to reintegrate the scientist's approach to nature's secrets, to try to bring a genuine multi-disciplinary point of view to bear on problems. However, we discovered that this was not as simple as it might seem. We tried at first to crowd the meeting with speakers representing different fields, as in the usual kind of meeting. It didn't get us very far. We tried to bring together the two or three men from different departments in one university who were working on the same problem. They didn't like it. Often they didn't even like each other. We then gradually began to focus more and more on what are the obstructions

between disciplines and now we look upon our conferences as experiments in communication and as efforts to identify and remove, if possible, some of these obstructions.

One thing that seemed to come out of this thinking was that there is an over-emphasis, derived, I think, primarily from our universities, on the intellectual side of learning and the intellectual side of science—and I mean intellect as opposed to emotion, the logical as opposed to the creative—and we felt that we would like to do something to redress the balance. We felt that the lecture system of the universities had reached a point where it was practically limited to the making of statements *at* people rather than being concerned with communication *with* people. This applies also to the papers at the ordinary scientific meetings, which are usually a series of statements made *at* people, statements which give great satisfaction to the speaker but little to the listener. Since the scientific programme is crowded, only one or two questions may be raised at the end of each statement, and then one goes on with the next paper. We wondered if something might not be accomplished if we reversed the process and tried to make the questions and answers, the discussion, the heart of our conference, and the papers as small a factor as possible. Gradually we pushed the papers back and brought the discussion forward so that now we have somebody who makes introductory remarks for about twenty minutes, which serve as the basis of discussion for the rest of the day. We tried to bring the discussions that ordinarily take place in the corridor outside the scientific meetings into the meetings themselves.

With respect to making statements *at* people rather than communicating with them, it seems to me that we should pay more attention to the listener. We should ask ourselves what kind of a receiving set the other fellow has got. Some of the difficulties of communication are linguistic; but others are to do with the imprint of authority, which makes it impossible for us really to hear a viewpoint which challenges the authority with which we have identified ourselves—sometimes it is our own authority. In days gone by I think it was possible for a person to remain relatively static for long periods of time, but today I think we must expect change. But change is something which produces anxiety, and anxiety is apt to be transformed into hostility. And I think that is one of the reasons why, when someone is challenged with an idea which would produce a change in his approach to the problem, so often the response is a hostile one. These resistances, these obstructions to communication, these distorting lenses, may change with different environmental situations. When the environment is a chill and anxious environment, the lenses go on—it's like the guard the boxer puts up. But if there's a warm

and friendly atmosphere, then the glasses come off, the guard is lowered, and the way is open for communication. That's one of the reasons why people must have an opportunity to be associated with each other informally. It's one of the reasons why having the same group, with a few additions, meet again and again and again often helps so singularly well to provide a basis for communication.

We try to encourage the participants in our meetings to speak freely, not to hesitate to ask a foolish question, because how can they be sure it won't evoke wisdom in someone else? We are not here in this kind of group to solve a point, to defend a view, but rather to examine our own blind spots, or at least to give others a chance to point them out to us. In this way we hope that we can open the doors to the kind of communication that previously hasn't been possible. This kind of meeting should be primarily for the cross-fertilization of ideas, for the meeting of minds, for the stimulation of curiosity, and for examining and bringing out the resources of the group. The resources of each of us are certainly not known to the others, and some of them possibly not to ourselves, because we have all stored many memories and experiences which we cannot bring to the surface at will on a given problem, but which may be there in the background of our memories, and brought out by someone else's remark.

We will now introduce ourselves to each other. I will start with myself and then ask each of you to say in a few words how you got the kind of interest that brought you to this meeting. I had an Hungarian mother, which has helped to give me a warm feeling towards the Continent, and a father of New England origin, and I'd like you to feel that when I am quite wild and woolly it's the romantic, dynamic, Magyar spirit bursting out and when I'm sensible and calm and intelligent then it's the New England spirit, which, of course, came from Great Britain. I graduated from the Harvard Medical School in 1921 and had a training in pathology and then another year and a half in medicine, and then went to the Department of Neuropathology in the Harvard Medical School under Dr. Stanley Cobb and did research on cerebro-spinal fluid, and on body fluids in general: respectable, solid, basic research. Then, when I was measuring cerebro-spinal fluid in a manometer, I discovered that even such a simple measurement as that could not be made accurately unless I had some knowledge of the emotional state of the patient, because if the patient was tense he might either hold his breath and give himself a high venous pressure, and too high a pressure in the cerebro-spinal fluid, or, on the other hand, he might react by over-ventilation, lower his venous pressure and get too low a cerebro-spinal fluid pressure; in this way I was introduced into psychosomatic

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problems. Then, since Dr. Cobb had many patients with epilepsy, I became very much interested in this problem and particularly in the factors which precipitated convulsions, and especially in emotional factors as one of the precipitating agents. And from there it turned out that the emotional factors which precipitated such attacks were very often factors of which the patient was unaware: old conflicts, for which the patient had an amnesia, so that I became more and more drawn from the psychosomatic into the psychiatric field, and although never properly trained in psychiatry nor properly analysed, I did have a personal analysis, which was very good for me, and some psychiatric training. Then I came into the Macy Foundation in 1936 and since then I have been in a curious situation. You all know the definition of the specialist who gradually gets to know more and more about less and less until he knows practically everything about nothing, and the generalist who gets to know less and less about more and more until he knows practically nothing about anything; but I am a mixture of those two, because I get exposed to an enormous range of conferences where the participants are groups of highly trained specialists.

MONNIER :

After medical studies in Geneva, Zürich, and Vienna, I worked in the Physiological Institute of Zürich, where I wrote my doctor's thesis under the direction of Professor W. R. Hess. I was already interested at that time in the correlations between psychological functions and the vegetative nervous system. From 1931 to 1934 I got a training in psychiatry as assistant at the Psychiatrische Universitätsklinik of Zürich, and from 1934 to 1937 a training in neurology at the Clinique des Maladies Nerveuses at the Salpêtrière in Paris. I went then as a Rockefeller Fellow to the Neurological Institute of Northwestern University in Chicago, where I studied the functions of the reticular system of the brainstem under the direction of Professor S. W. Ranson. During 1938, I worked as assistant in the Service de Neurologie de l'Hôpital Cantonal de Genève and in 1939 I became Chef des Travaux at the Institut de Physiologie de Genève.

In 1941 I went to Zürich as Chef des Travaux at the Physiological Institute, where I worked again with Professor W. R. Hess. I was interested in the integrative activity of the nervous system and had the opportunity of studying, with Professor H. Willi, head of the Kantonales Säuglingsheim, the development of motor functions in normal and anencephalic newborns. In 1948, I returned to Geneva to start the Laboratoire de Neurophysiologie appliquée, which had

as its purpose the bridging of the gap between experimental and clinical neurophysiology. Here I had the opportunity to develop with Professor Piaget and Mile Inhelder a co-operative study of the correlation between mental development as analysed by various psychological tests, and the development of the electrical activity of the brain, analysed by means of electroencephalography. This is one of the reasons why I am here.

STRUTHERS:

I don't know of anyone who is in this group who has less right to be here than I have as I am not trained as a scientist in any particular field. I had some experience in the army in the first war as a physician and also in general practice in the country and I eventually entered paediatrics, in the days when most of paediatric work was a question of whether you had sufficient lactic acid in the baby's feed. After twenty years in general paediatric practice in Montreal, during which I did some work on childhood tuberculosis as a social problem, and on the definition of activity of rheumatic fever in children, I found that my main interest was in undergraduate medical education. Because of that interest I am now dedicated to work in the Rockefeller Foundation as Director for Medical Programmes in Europe. The only justification for my attendance here is that I can qualify under Dr. Fremont-Smith's definition of one of those who knows less and less about more and more, and I think I have almost reached the apogee of knowing nothing.

TANNER:

I am a human biologist, with particular interests in physiology, child development, and genetics. I had originally a mathematical training at school, with the intention of becoming an army engineer. However, this project had never really appealed to me, and a few days before it came to fruition I deserted and began a pre-medical course. This I greatly enjoyed and soon I had a particular interest in genetics and evolutionary theory. Early in the war I was a medical student and the Rockefeller Foundation generously offered to take some of us from Britain over to America and to train us there because of the difficulties we were facing in the hospitals in London. I had at that time done a small amount of research work in pure physiology and my interests by this time had become, if not focused, at least fairly concrete, so that when I was asked about them at the interview associated with this trip, I said 'I want to work in the place where physiology, psychology, and sociology meet'. This is a stand from which I have never really departed. So, having had my anatomical

and physiological training in England I had my clinical training at the University of Pennsylvania, and later on as a house physician on the medical service of the Johns Hopkins Hospital.

Following that, I came back again to England and being now quite clear that I wanted to become, if necessary all by myself, a human biologist, I spent the next two years in psychiatry, dealing almost exclusively with the superficial psychotherapy of neuroses of a combat type. I was particularly concerned at that time with group therapy. At the end of this period, having learned just enough about psychology and psychiatry to understand the language, I went to Oxford University as lecturer in Physical Anthropology. I did that because it seemed to me that if one was concerned with studying human biology and behaviour one should build one's science from the ground up, studying first of all the simplest thing there is to study in the human, which is what he looks like.

At Oxford I spent a great deal of time getting to know the literature on the physical growth of the child, and trying to define and classify differences in physique among adults. I think the interest of these physical differences lies in the light they may throw on differences in physiological function and in behaviour. After I had spent three years at Oxford learning, by the well-known mechanism of teaching, the subject of physical anthropology, I went back to be a professional physiologist at St. Thomas's Hospital in London University, where I now teach physiology and do research work on the physiological differences between people, their genetical basis and their anatomical correlates.

There is one other thing I should tell you. I feel very likely that my best function here may be more to answer questions that the ethologists and the psychologists may bring up than to propose anything of my own, and therefore the more I tell you about the fields in which you may reasonably expect to get a sensible answer out of me the better. For the last four years I have been closely associated with a study on human growth run and financed by the Ministry of Health outside London and here we have a longitudinal growth-study, the first of its kind in England, modelled to a considerable extent on those which are doing such good work in the United States. We have there about 250 children and a group of us, collected mainly from various departments of London University, descend on these children two consecutive days every month. We therefore see every child within two weeks of its birthday or half-birthday; we see every child every six months, or every three months during puberty. At the present time we do a number of physical measurements of the children, we take photographs in a rather highly standardized and particular way which enables us to measure them, and we take a considerable

number of X-rays, the function of which is to differentiate bone and muscle and fat so as to see the growth of the different sorts of tissue in children. We have dental people who take X-rays of the jaws and are concerned with the development of the teeth; and a paediatrician who does the clinical examinations. We are dealing with bone-ages, teeth-development-ages and such-like things, subjects in which you may be interested. We do not have—and I regard it as a very grave gap in our investigation; it is a matter of money and space as usual—anybody who is studying the physiological and biochemical development of the children, so I have no personal experience of that. However, I am quite well acquainted with such literature as there is on it. Also we do not have at the present time any psychiatric or psychological studies in progress, and this is a field about which I wish to be informed, and know, practically speaking, nothing.

MEAD :

I started my training as an anthropologist at the age of about three, which gives me some sort of qualification for working on the problem of children. My mother was a sociologist who was working on the adjustment of Italian immigrants in the United States, and my grandmother was very much interested in child thought and imagination. She was a teacher of young children. So I was trained on my younger sisters to record children's behaviour, and by the time I was nine or ten I was a fairly competent recorder. We don't know yet whether starting that early does any good or not, but I went on a fairly straight line, having come out of this sort of academic expectation. I took my M.A. in psychology and worked initially on the effect of language spoken in the home of immigrants on their performance in intelligence tests at school, and that meant working through the literature on the relationship of intelligence testing to race. I then went into anthropology under Franz Boas. I suppose I belong ethnically to that very small and rapidly vanishing minority called 'Old Americans', as I have no ties that can be traced to Europe. I am tenth-generation American, so that Europe to me is a strange and new place that I have come face to face with, with very little chance of following any ties back.

In my anthropological work under Franz Boas I gained integration with European scientific thought, because Boas always gave us the first reference in German, the second in French, and sometimes one in English. My first anthropological work was on the extent to which the phenomena of adolescence could be regarded as biological and the extent to which they were culturally influenced. That was 1925. Since that time I have worked in eight different primitive societies,

taking to them the problems that were developing in the field of human behaviour over that period. During my second field trip in 1928 I took Professor Piaget's early work to the field and attempted to test it out. I am now going back this June, twenty-five years later, to re-study the same community, and I will be able to take this next twenty-five years of Professor Piaget's work with me. In the same way I have used from time to time one set of psychological formulations and at another stage some other or later development.

During the last twenty years I have been under the protective aegis of the Macy Foundation and all its cross-cultural and cross-disciplinary and cross-everything enterprises. I have been exposed to a very large number of conferences such as this, which have brought new problems, which one could use to take back to the field. As Dr. Tanner said of himself, I may be more useful in answering questions than in presenting material, because one of the characteristics of ethnological work is that we deal with whole cultures and so many varieties of material that it would be rather difficult to anticipate just which kind of material would be most useful. At the same time the anthropologist can take any hypothesis back into the field and subject it to new tests. My major function in these conferences was first to say whether the hypothesis was culturally limited or not. Is it physiological to nod your head to say 'yes', and did the negative head shake come from the child avoiding the mother's breast? Is it culturally stylized? Secondly, to present hypotheses which come out of the material. In most cases primitive material cannot supply proof. We deal with too small populations over too short a period of time. We are a hypothesis-criticizing, -correcting, and -producing agency rather than a proof-producing agency.

GREY WALTER:

Well, sir, I may add to your confession of cosmopolitanism that I have an American mother and an English father, and that I share a birthplace with Norbert Wiener, T. S. Eliot, and Harry Truman. I was born in Missouri. For that reason my life has been one long illustration of the need to 'show me'.

I am an experimental scientific worker. I started my training in the University of Cambridge as a fairly pure neurophysiologist in the school of Adrian and Matthews, and I spent five years there, studying the detailed neurophysiology of the peripheral nervous system. I then had the honour of being delegated by my professor, Sir Joseph Barcroft, to work with a Rockefeller Fellow—one of the first and few who came from Leningrad—on conditioned reflexes. I was given the task of acting as his assistant and becoming familiar

with the classical techniques of the Pavlovian School. I spent two years at that work, having a good background already of neurophysiology. I was enabled first of all to introduce a number of modernizations into the Pavlovian technique, to assure myself of the essential accuracy of the Pavlovian hypotheses, and to become much impressed with the manner in which the Pavlovian workers at that time were able to distinguish factors related to personality in their experimental creatures, both animal and man. Since that time, as you know, that particular aspect of Pavlovian work has been rejected and denied by the Soviet authorities, and very few people, I think, understand how important the typology of Pavlov was, in the early days, to the development and scope of the Pavlovian theories.

After we had realized that to extend the work in Cambridge would cost far more money than was available, I had the good fortune to be appointed as a Rockefeller Fellow at the Maudsley Hospital in London, where my approach to the human problem was directed and inspired by Professor Golla, who was then setting up a new laboratory for the multi-disciplinary study of the human organism; I had the role there of physiologist. There I was introduced to the study of the electrical activity of the brain, which as a physiologist I had previously considered to be inaccurate and unlikely to lead to any information, the brain being, of course, at that time a most objectionable subject of study. I had the opportunity to visit many European centres of brain physiology preparatory to setting up our own laboratory. I met Berger and Foerster and various other workers in the field of brain physiology. Our laboratory was set up mainly for the application of electroencephalography to psychiatric problems, but we were very soon more heavily involved with neurology, and I devoted a number of years to the study of organic lesions of the nervous system. It was rather a tough apprenticeship for a physiologist, having to relearn neuroanatomy and apply it to what was then an extremely inaccurate and troublesome method of study.

At the end of my period at the Maudsley, just before the War, I moved with Golla to Bristol, where I am now, and once again had to redirect my ideas towards the more generalized physiology of the human nervous system. Our plans were interrupted by the War. During the War we devoted our attention mainly to the problem of head injuries and epilepsy in Service personnel, but at the same time occurred the opportunity to deal with more normal physiology in matters quite relevant to the meeting here, that is the problem of children evacuated from the cities. Hundreds of ill-behaved and, in fact, horrible creatures descended upon us from the slums of big cities, presenting one of the most serious problems which my country has had to face: the disposal of these young creatures in



schools, billets, and so forth. We found that the application of physiological techniques to the separation, selection, and classification of these children was astonishingly valuable. From that time dates my interest in the relevance of the physiology of the nervous system to the study of how children grow up, how the influences of environment and heredity, nurture and nature, combine to make the child as it is.

These interests have been paramount in my scientific thinking, combined obviously with the early influence of the Pavlovian School, and I have attempted particularly to quantify methods of study, to develop men and machines able to make objective and concrete appreciation of the problems which we encounter in this sort of work. This approach seems to me to have been neglected in the past, and ignorance here is liable to produce considerable misunderstanding if projected further.

ZAZZO:

As my name suggests, I am descended from Italian stock on my father's side. I was born in Paris in 1910 from a Burgundian mother and a Parisian father. My early teachers were Piéron and particularly Wallon. In 1933, having finished my studies at the Sorbonne, I decided to go abroad. Chance, or rather political events, led me overseas. I intended to go to Vienna to work with Freud, but the circumstances were not very favourable so I decided, while waiting for the situation to improve, to go to the U.S.A., where I worked for six months with Gesell in his Institute at Yale University. I came back to France in the Spring of 1934 and it was then that I started work directly under Wallon.

In 1940 I published my first book, which was devoted to American psychology. This was also at a difficult period, and the following incident is worth quoting. French censorship, under German control, required me to delete all the Jewish names from my book. Naturally this was rather awkward. I solved the problem by taking the names out of the preface and putting them back in the following chapters; and so I was able to ascertain that the censorship did not read beyond the first chapter. At this time, in 1940, I was working at the Psychopathological Laboratory at the Henri Rousselle Hospital.

Four years ago I took over from Professor Wallon as Director of the Laboratory of the Psychobiology of the Child. My present research bears more particularly on the psychology of epilepsy. Our team of workers is dealing with the classical problem of mental deficiency, certain types of which we are trying to redefine. It is an old problem but always seems new.

For the last few years my 'hobby-horse' has been the study of the foundations of personality, using various methods, especially with twins.

Finally, on the question of method I would like to mention that I have attempted to reconcile the clinical method taught by Professor Wallon and the statistical, quantitative, approach. There are difficulties, which are, moreover, exaggerated by doctrinal oppositions and opposition between groups which regard themselves as strictly clinical or strictly psychostatistical; but, as I say, I endeavour to reconcile these two points of view, both of which seem to me essential for the understanding of human nature.

MELIN :

I am a paediatrician. After my medical examination I studied paediatrics in Stockholm, and very soon I was directed by my chief, Professor Lichtenstein, towards the field of children's convulsive disorders. This was at a time when electroencephalography was not at all known as a clinical method in Sweden. It was not possible to learn about it at home, and that led to my visiting the United States, where I studied at Harvard Medical School, mainly in the clinic of Lennox, and also at Johns Hopkins Hospital and the Neurological Institute with Hoefer. I went back to Sweden again and continued in this field. However, I found it very difficult to judge pathological conditions, knowing almost nothing at all about the normal in children. It has therefore been a constant struggle during the years to find out what is normal in children, especially in the field of the electrical development of the brain. My interest has been mainly in that direction and I have tried to work in this field from the electroencephalographic point of view. I am, however, still a pure paediatrician, and I have still my main interest focused on work with children suffering from convulsive disorders.

RÉMOND :

I began my studies in Picardy, in the north of France, and continued at the Faculty of Medicine of the University of Paris. There I was particularly interested in the study of the nervous system of the adult and the child. One of my best-liked tutors, Professor Baudouin, was the first in France (in 1936) to become interested in electroencephalography. In 1939 I was already thinking of concentrating on neurophysiological research.

In 1941 I started working regularly in Professor Baudouin's laboratory, where I studied mainly the electrical activity of the brain of sick children, with the assistance of Professor Heuyer and Professor

Debré. I was lucky enough in 1945 to work for a year in the U.S.A., in Professor Detlew Bronk's laboratory at Philadelphia; there I was able to become familiar with the techniques of neurophysiology. Although these techniques had perhaps not entirely escaped notice in France during the war, little was known of the details. I was able to study particularly the polarographic recording of oxygen and apply it to the study of cortical metabolism.

Since 1946 I have been with the Centre Nationale de la Recherche Scientifique and have concentrated on the electrophysiology of the brain.

I went back to the U.S.A. in 1952 to work with Professor McCulloch in Chicago, and since my return I have again held the post of Head of the Laboratory of Electroencephalography and Applied Neurophysiology. As a daily routine in this laboratory we make electroencephalograms of about twenty persons presenting different lesions of the brain. Having made most interesting contacts at the Salpêtrière in a neurological milieu which is already long established, I want to profit by the atmosphere which exists there to base on it research in human neurophysiology.

Regarding the research which I have followed or carried out myself, I should like to mention that in our laboratory we are attempting much more than in the past to 'define' the normal individual. Since we are constantly working with sick persons we cannot always know where the pathological begins and the normal ends. What is the normal individual? What is the normal child? One might ask whether normality exists.

Among the groups of people we try to study I should like to refer to a very restricted one, of pilot apprentices. In order to show the difficulties we are faced with I would mention that individuals of twenty to twenty-four years in this group who appeared absolutely normal according to screening, definitions and tests, seem to us, used to observing adults, to be immature as regards the electrical activity of the brain. I mention this fact before we start on our work in order to ask each of you to underline as far as possible what you consider normal and to tell us what importance the definition of normal has in the fields you represent. How can we obtain this definition? Why should we obtain it?

KRAFF:

I think I might, with due apologies to Dr. Lorenz, describe myself as some sort of bird of migration. I was born and studied medicine in Germany and took my degree at Leipzig University, and then I started on my first Transatlantic migration, to the Argentine, from

which I came back in order to concentrate on neurology and psychiatry at the University of Munich, at the University of Paris, and for some time also in Zürich. Eventually I wound up being a lecturer in psychiatry and neurology at the University of Cologne, where I remained until Hitler came into power and, as Hitler and I couldn't see eye to eye on many things, I decided that I'd rather withdraw to South America again, and so I settled in 1933 in Argentina. Since then I've commuted between South America and Europe. Eventually I became an Associate Professor of Psychiatry in Buenos Aires, and somewhat belatedly the University of Cologne also conferred the title of Professor on me.

As to professional experience, there I think I am a bird of migration too. I started on neurological lines, did a certain amount of what is called solid research on the neurological aspects of psychiatry, until I found out that I couldn't do without some other sort of training; so I underwent my personal psychoanalysis, and since then have been commuting between neurological and psychoanalytic psychiatry. I feel that birds of migration in a way have some sort of stability, they always fly by the same route, and if there are two schools of psychiatry—one in which one tries to explain everything in physiological terms, and the other one where everything is couched in psychological terms—I have always been most interested in the gap in between, which is in my opinion neither quite as wide as some people seem to think, nor quite as narrow as some others believe. I think it is a most fascinating task to see how things can happen in physiology through psychological stimuli, and how psychological events use physiological channels in order to manifest themselves, and in this context lately I have been interested in the 'gap' in epilepsy and the convulsive states generally, and in the so-called psychopathic personalities, two subjects which are particularly closely related to some of the most outstanding neurophysiological problems in children. Lately also I have been particularly interested in the implications of speech pathology from the point of view both of psychology and brain pathology.

BOWLBY:

I am a Londoner born and bred. My father's family came from Yorkshire, my mother's from Wales, and my father was a surgeon. I had rather a wayward youth, inasmuch as I toyed with being a sailor and went to the Royal Naval College; I then took up medicine, then switched to child psychology, and it was unfortunate that it was at my most wayward that my father died. This was at a point when, after reading natural sciences, medicine, and psychology at

Cambridge, I decided to give up medicine in order to take up education. I spent twelve months in one of the progressive and free schools, which was a very valuable experience, because I saw a number of disturbed children at first hand, I lived with them, indeed I had to look after them, and I met there the first 'affectionless character' of my career.

Fortunately, I was very wisely advised at this point to finish my medical training and train in psychoanalysis. So I went to London, to University College Hospital. Then I specialized in psychiatry at the Maudsley Hospital and continued my training in psychoanalysis; I finished that and took up child psychiatry and child guidance. Between 1936 and 1940 I was concerned with child guidance and it was really at that time that I became convinced in my own heart that certain events of early childhood were of critical importance in determining personality development—particularly the child's relationship to his mother, and the mother's unconscious attitude to the child, based on her own childhood experiences. I ought to say that my concentration on the mother-child relationship was largely due to the influence of Melanie Klein. Now, I was eager to make scientific these clinical observations on mother-child relationships, and I seized on the particular relationship between the experience of a child being separated from his mother and the psychopathic affectionless character, not because it was the most important, but because it seemed to me the most concrete and the simplest to study.

Then came the war. I spent five years in the army as an army psychiatrist, and much of my time was spent in officer-selection work. I received a post-graduate education in psychology in the army and a training in research method. I also learnt that the way to get people of diverse backgrounds and disciplines and outlooks to work together was to give them one single task.

After the war I was offered a full-time post at the Tavistock Clinic, where I have been for the last seven years, in charge of the Child Guidance Department; there I have had one foot in the clinical field and one foot in research, trying to 'scientificate' the clinical field. I think when I returned to child guidance after the war I did so with some doubt as to whether my clinical convictions of pre-war days were going to stand up to further scrutiny, and I was rather delighted to find that they all did, or at least they seemed to.

I returned very swiftly to my hobby-horse, mother-child separation. It seemed to me that it was one of the few islands of dry ground in a rather swampy scientific field, and that one had here a definable experience which demonstrably could sometimes produce a particular type of personality outcome. I have stuck very rigidly to it, with two

or three purposes in mind. The first has been to substantiate a claim that all child psychiatrists make, that these early experiences between the parent and child are really as important as we think they are, in contrast to the view of many who ridicule it. The second purpose is to make clear that here is an aetiological factor calling for preventive action in the mental health field, something clear cut and concrete that people can get hold of. The third purpose is research; I felt that we had here a scientific phenomenon to the study of which many techniques could be brought which might lead to a unification of different points of view. My own techniques were those of psychoanalysis and child psychiatry but I hoped to bring to bear on this one single problem a variety of disciplines. It was my good fortune in 1950 to be invited by W.H.O. to read the literature on this subject and really get to grips with it. Since then I have been interested in getting hold of any scientific knowledge which seemed to lead to an understanding of why it is that that particular sort of experience, the child being separated from his mother for many months, could not only have an effect on the character but have a *permanent* effect. What is the cause of the permanence? Well, that led me, amongst other things, to be interested in Professor Lorenz's work; the phenomenon of imprinting at once struck me as possibly important to my work. Whether it really has anything to do with the effects of separation we shall see. The other thing that fascinated me in his work was the mother-child relationship of animals. The mother-child relationship is manifestly an example of instinct, in the ethological meaning of the word, and it is also at the centre of psychoanalysis.

Other things which have been interesting me more recently have been the phenomena of behaviour under stress and experimental neurosis because, here again, one is faced with peculiar forms of learning which have a remarkable quality of persistence. My interest in this Study Group is the hope that some other disciplines could help me in my quest. I must confess to a rather one-track, one-problem mind.

LORENZ:

I am born and bred Austrian, from near Vienna. My scientific career, rather like that of Dr. Mead, started at the age of five, when I got a nest of ducklings as an Easter present, and I may say that the ducklings and I became imprinted upon each other, and curiously enough the history of my teacher Heinroth, who has done most of his work on *Anatidae*, begins in exactly the same way, with a gosling, not a duckling, but also at five years. Well, that may be a coincidence. Then, at about twelve years, I became acquainted with Charles

Darwin, by means of a popular booklet (DARWIN, 1872), and from then on I became a passionate evolutionist, and besides started to become a comparative zoologist, which my father, who was an orthopaedic surgeon, tried to prevent by ordering me to study medicine. This afterwards saved my life, when I was in Russia. Nevertheless, I resented it strongly at the time, and when I was a graduate student, I attached myself to Professor Hochstetter, who was the best morphologist and comparative anatomist living, and decided to become a comparative anatomist, not changing my preference for evolution, though. I did not give up keeping live animals, but regarded it as a hobby, a plaything. Now, at Hochstetter's chair, I learnt how one ought to proceed in investigating and reconstructing the course of evolution. Then I learnt among other things one fundamental sentence, which my teacher Hochstetter used to repeat again and again, 'Primitive animals do not exist, only primitive *characters* do.'

Then I made my discovery; it wasn't mine, it was actually Charles Whitman's, but I didn't know it. I discovered instinctive movement. I discovered for myself that innate movements are just as conservative characters in the species or genus as are any claws or bones. The museum zoologist is apt to think that what is most resistant and keepable in the museum must be so in evolution, but it isn't true.

Then, when I was just full of the first discovery, I discovered Heinroth, and I saw that he knew the same thing, and I found out that comparative morphology of movement might be worth while studying. At the same time I was not, and Heinroth was not, interested in the physiology or in the psychological importance of instinctive movements. We were interested in comparative characters. We were interested in finding more and more *characters* that could be used in reconstructing evolution. And simply because there were not enough morphological characters available, we proceeded to spread our search to behaviour patterns and found that these very often are even more conservative and reliable landmarks of phylogeny than the colours of plumage or the form of bones. In view of the fact that instinctive movements were discovered in this particular way it always seems somewhat paradoxical to me that there still are people who try to deny their very existence.

And now on the strength of Heinroth's authority, I decided to make my profession out of what had been my hobby up to that time in the study of animals, and I quit my position as an assistant in the anatomical institute, and became docent in Vienna for comparative psychology. At the time that was impossible, because in Vienna the very Catholic régime prohibited animals having souls and therefore a psychology of animals was impossible; and so I got a lectureship

for comparative anatomy and—hush-hush—psychology. I have never delivered a lecture on comparative anatomy in my life. Then I became professor of psychology in Koenigsberg, together with the pragmatist philosopher Eduard Baumgarten, a disciple and ardent admirer of John Dewey. Together we had a great number of good fights with Neo-Kantian philosophers, and, in general, it was a very nice collaboration between philosophical anthropology and ethology.

For a very short time I became a soldier and then was recruited as an army surgeon because I had a full doctor's degree of medicine. From then on I worked as a psychiatrist and neurologist, having been promoted to the rank of an Unterarzt, which is a sergeant. I was for two years in Posen, under a very intelligent teacher, Dr. Weigl, who was interested in the study of neurosis. It was interesting to me that neurosis developed in people who had an instinctive inhibition towards killing. Such people were my patients because they couldn't kill any more, and what struck me as most gruesome was that there were so few of them—one would have expected many more. Then I was transferred to the front and immediately caught by the Russians, and then I went on being a neurologist and a psychologist in Russia, without any interruption. There I was treated very decently indeed for four years and wrote a volume of my textbook (LORENZ, in press) there, and was able to bring it home. Then I came to Vienna and again developed my little Institute of Comparative Ethology in Altenberg, with the help of the Austrian Academy of Sciences, which was financed by the English poet and writer, J. B. Priestley, who gave all the royalties of his plays and publications in Austria to the Austrian Academy of Sciences. My work went on very prettily on that but the family finances did not; we had to sell one piece of ground after the other; Altenberg became smaller and smaller. Then I got a call to the University of Bristol, where I was to do comparative ethological work on the beautiful collection of ducks and geese collected by my friend Peter Scott, and simultaneously I got an appointment by the Max-Planck-Gesellschaft to take over a newly-established Institute, and I decided to take the latter, although after very much hesitation.

Our present work tends to become interested, as you see, in knowing more and more about less and less; the problems begin to specialize more and more and therefore I think it is very good for me to take part in this meeting to widen my interests again. It is a great satisfaction to me that comparative ethology is something that child psychologists and psychiatrists begin to get interested in, because what we do is only to create a base; whether you can use it is something beyond my knowledge. Well, I hope you can.

BOWLBY:

May I ask one question? I would like to know when the term 'ethology' was coined, and by whom.

LORENZ:

The term ethology was created by Heinroth, who, with Whitman, was the pioneer of that science. It's an interesting fact, though, that neither knew of the other's existence, still less about his work; Heinroth called one of his first and most important papers 'Beiträge zur Biologie, insbesondere Psychologie und Ethologie der Anatiden' (1911). The subject of this paper is constituted by the *innate* activities and reactions of the birds in question. Tinbergen took over the term ethology, which I consider rather a pity, because it creates misunderstandings with psychologists and philosophers. Neither Heinroth nor Tinbergen cared in the least about human psychology and philosophy, sciences in which the words ethos and ethics have a very different meaning. So they did not mind the ambiguity of the word ethology. But I think it is too late to do anything about it now, we are called, we are branded, 'ethologists'. I never use the term ethology in German, though; I say '*Vergleichende Verhaltenslehre*'. The German ending *-lehre* has the advantage over the English *-ology* that you can join it on to practically anything. You could not say 'Comparative Behaviourology' in English—and that is what probably influenced Tinbergen. But the word ethology is really awful, I concede that to any psychologist.

INHELDER:

I am Swiss, and spent my childhood and adolescence in eastern Switzerland. Being the daughter of a zoologist I amused myself, as naïvely as many other children do, rearing various animals, including tortoises, without in the least suspecting the role Dr. Grey Walter would one day make them play in cybernetics. I went to the University in Geneva where I studied under Professors Claparède and Piaget.

At the Institute of the Sciences of Education in Geneva it is common practice to throw students into the water to teach them to swim. So from the very beginning M. Piaget asked me to participate in his work and assigned me a piece of experimental research; it was a matter of studying how a child forms the idea of physical conservation. M. Piaget proposed that I should do this by dissolving pieces of sugar in water to see whether the children believed in the destruction of matter or supposed that it was conserved, and to see what atomistic intuition they had (PIAGET and INHELDER, 1941).

At the beginning of the war I was asked to establish in German-speaking Switzerland a centre for the psychology of school children with the main object of discovering and diagnosing cases of mental retardation. The psychological research on development I had carried out previously with a purely scientific and theoretical aim was then most useful to me. The stages in the genesis of the concepts of conservation supplied me with a scale of development on which the phenomena of mental deficiency showed as delays and fixations (INHELDER, 1943). In my case-finding investigations, which led me from one school to another, I resolutely devoted to a scientific cause the few pieces of sugar allowed per month at that time!

In 1943 I was recalled to the Institute, to act firstly as 'Chef des travaux', then as 'Chargée de Cours', and from 1948 as Professor of Child Psychology. My main activity, however, is still directed towards research.

With a group of assistants I train the young students in the use of scientific and clinical methods by allowing them to participate in our work. In recent years we have concentrated on the genesis of spatial concepts (PIAGET and INHELDER, 1946; PIAGET, INHELDER and SZEMINSKA, 1948) and of physical and mathematical concepts (PIAGET and INHELDER, 1951). At the moment we are hoping to conclude a series of studies on experimental and inductive reasoning in children and adolescents (INHELDER, 1948, 1951).

In recent years we have been working in a highly specialized, restricted area: that of the genesis of intellectual functions in children. This specialization was a result of the need to push our research deeper, but until it rests on a neurological basis and as long as we remain ignorant of the emotional and social background of the child I consider it to be hanging in mid-air. As I wish to incorporate our results in a comprehensive study I welcome the possibility of collaborating with Dr. Monnier and Dr. Odier. I am particularly glad of the opportunity of participating in this meeting and hope not only to obtain information but also to make new plans for research.

PIAGET:

I was born at Neuchâtel, in Switzerland. I was less precocious than Dr. Mead and Dr. Lorenz because I was already fifteen when my first work was published. This work dealt with a special field of zoology—the study of terrestrial molluscs.

The way I came to study child psychology was far from orthodox. I had studied natural sciences, and my doctor's thesis dealt, of course, with molluscs. What interested me most were the problems of adaptation, of the relation between an organ and its environment and the

problem of variation as a function of environment and as a function of structure. But while I was preparing my doctorate in zoology I was taking a very lively interest in the problems of knowledge, of epistemology, of logic, and the history of sciences, etc. At the same time I was highly suspicious of philosophers who I thought had treated the problem of knowledge in a fashion that was far too speculative and not sufficiently experimental. I then considered devising a genetic theory of knowledge, studying knowledge as a function of its growth and development, so I felt I should read psychology. I thought I would spend four or five years studying the development of logic and the intellectual functions in the child and the growth of intelligence during the child's development; these studies have lasted more than thirty years and are not yet finished.

To begin with I concentrated mainly on the problem of logic in the child, then on the concepts that are basic to science: number, space, time, etc. My method of studying logic in the child was much too verbal at first, dealing particularly with the relation between thought and language. Gradually I discovered that for this study it was essential to go back to the actions themselves, to the reasoning which is carried out not through language but through manipulation of objects. Starting with my books on the first and second years of child development my technique has always been to study reasoning through objects set up so that the child could make certain experiments.

This study of the child's actions brought me to a conception of logic based on operations—an operation being considered as internalized action which becomes reversible, that is to say can be carried out in both directions, and links up with others. In the sphere of intelligence operations always constitute whole structures, rather like the Gestalt in the sphere of perception; the structures being, however, larger, more mobile and essentially reversible, and capable of co-ordination. For several years we have studied these structures in the infant, then in the child of seven to twelve years, and, finally, thanks to the recent work of Mlle Inhelder, in adolescence. These structures are of great interest. As one arrives at a certain degree of generalization in the study of operative structures one finds again the fundamental structures of mathematics; algebraic structures such as the group, or structures based on the idea of order, such as the lattice concept, and topological structures, etc. We are studying the achievement of these structures in the adolescent and their development during childhood, and they seem to give some hope of co-ordination between psychology and neurology. It is evident that such general structures are based on the activity of the brain. Although perhaps no neurological contact is possible at present, except for attempts—such as those of Pitts and McCulloch—to

apply logical structures to neuron structures, I think nevertheless that now we can go further in developing the comparison between the various cybernetic models and the activity of intelligence, as we have tried to define it. Moreover the attempt to discover all possible connexions between these fields comes within the frame of reference of this meeting.

ODIER :

Dr. Mead told us that she began her career at the age of three. By comparison I feel like a newcomer, because my entry into the corporation dates only from my Oedipus complex, at the age of five or six.

I had the rare good fortune to have a father who was most interested in all the questions I asked and took the trouble to reply. I went through the phase called 'questionnisme' in French. As you can imagine I gradually realized that my good father did not know everything, that many problems remained unsolved, and I thought it was the duty of my generation to study them more closely. That is the origin of my vocation, for all my questions were connected with the thoughts, intentions, and opinions of grown-ups.

I will pass over a few years and come to an important event which occurred during my medical studies. Professor Flounoy gave a summer course in psychology and I had the privilege of making his personal acquaintance. I would mention that Professor Flounoy was the first person from a French-speaking country to become interested in a certain method, a certain psychological conception, taught at that time by a Dr. Freud in Vienna.

Later, after the first war, I spent two years in the department of neurology at the Salpêtrière, mainly under Professor Pierre Marie and Professor Souques, who was Charcot's first pupil. I worked in a section for wounded soldiers where cases of mental shock were unfortunately fairly frequent. I saw several cases of post-shock syndrome psychoanalysed for months or even years with truly remarkable results. The patients recovered despite the opinion of the psychoanalysts, who considered the aetiology of the syndromes to be purely psychogenic. Because of this period spent in Paris I always think of the possibility of faulty diagnosis and consider the delicate question of physiogenesis or psychogenesis.

Later I studied in Vienna. It was then that I deviated. I was going to be led astray into psychiatry. One day, looking through the programme of courses, I found that Freud was giving a course on the theory of instinct. Remembering my conversations with Flounoy and Claparède, I decided to follow this course. There were ten or eleven students—twelve on a good day.

After that I worked in the Psychoanalytic Institute in Berlin. I was psychoanalysed and received psychoanalytic training. My teachers were Freud's first pupils: Sachs, Abraham, Rado, and even Jung.

I spent another period in Paris where I had been asked to contribute to the creation and organization of the Institut de Psychanalyse and the *Revue Française de Psychanalyse*.

Gradually, however, I became aware of what I might call a lacuna in Freud's theory—the psychology and activity of the ego. At that time it was very bad form to mention the ego in a psychoanalytic discussion, but I discovered that this evaluation was only a resistance, or rather a defence mechanism; actually my colleagues certainly felt that something was wrong. It must be admitted that data on the ego in Freud's theory were precarious, insufficient and sometimes even contradictory. Therefore in Paris, in order to try to bridge this gap, I turned to the work of the genetic psychology school and plunged into the works of Piaget. I was immediately struck by the many connexions between neurotic thinking and all the mechanisms described by Freud, and infant thinking as described by Piaget and his followers. In my course at the Institute of Psychoanalysis I tried to establish links between stages of development of thinking and intelligence as described by Piaget and instinctive-affective stages described by Freud. I immediately ran into great difficulties, I found that it was exceedingly difficult to connect these different stages, as if the child adopted either the theories of Freud or those of Piaget, developing in one direction or the other.

However, there are two points on which I think a correspondence exists, and which play an exceedingly important part in the psycho-biological or biologico-psychological development of the child, particularly in this highly complex evolution which should culminate in the socialization of the individual. These two points are: firstly, everything connected with the mechanisms of the super-ego and, secondly, everything connected with the well-known Oedipus complex. The latter, in my opinion, is a most important and critical phase in the development of the individual; I would almost say a second critical phase, admitting the great importance of the first critical phase so well described by Dr. Bowlby. The Oedipus complex is not a sign of illness; if it develops normally it is, on the contrary, a factor making for balance and development.

CAROTHERS:

It is perhaps relevant to my career that I was born in South Africa and lived for several years of early childhood in South Africa, and had an African nanny. Then I went to England, was educated there,

and so to hospital and took a medical degree about 1926. After qualification, I went to Kenya Colony as a medical officer of the Government and for nine years worked as a general medical officer throughout all parts of the Colony. I did medicine and surgery and midwifery and saw the people in their homes and in the hospitals and the out-patient departments of the little hospitals throughout the Colony, and throughout those nine years I must have seen a number which runs into six figures of the population of Kenya.

Round about 1937 the Colony had developed to the point where it was considered necessary for a medical man to be permanently attached to the Mental Hospital. There had been for some time before that a mental hospital, but it had been regarded only as a place where any doctor who happened to be near at hand could look in from time to time and deal with emergencies. There were two schools of thought in this matter—one school thought that they should get an expert from England who could study the African and the African language after he arrived; the other school of thought felt that they should get somebody from the Colony who knew the people and the language and could take his degree in psychiatry at a later time. The first school won the day and an expert was brought from England, but this arrangement did not last very long, and nine months later I was called upon to fill the breach. I don't know to this day why I was chosen, but I accepted the post and after taking it on I became more and more intrigued and interested in the work; but I had no opportunity to return to England for many years, because the war intervened, and I had to do my psychiatric study of the people on the basis of textbooks, and of talking to occasional people who knew something about the subject and who were passing through the country, and I found myself in very great difficulties over diagnosis, and finally decided that I must be a very bad diagnostician.

However, the opportunity came in 1946 to return to England and take the Diploma of Psychological Medicine in London, and then it dawned upon me that my difficulties were partly due to the fact that the pictures of disease as described in European textbooks of psychiatry are not at all completely applicable to the pictures that one sees in Africa, and this has been the theme of my interest for the last several years—are these differences real and if so what accounts for them. In regard to this meeting on the psychobiology of the child I should say that it seems to me that in many ways the African, as an individual, stops short at the second stage that Professor Piaget has described and I would like to know why this is so.

FIRST DISCUSSION

Physical and Physiological Aspects of Child Development

TANNER :

I am sorry to say that during the next fifteen minutes I am going to make a considerable number of statements without giving you the detailed evidence for many of them. It seems to be the only way I can effectively get across what I have to say, which will really constitute my feelings about the growth process, based entirely on morphological and physiological growth and not on the psychological aspects, about which I have no personal experience. These ideas may or may not clash with the ideas coming from the psychological side.

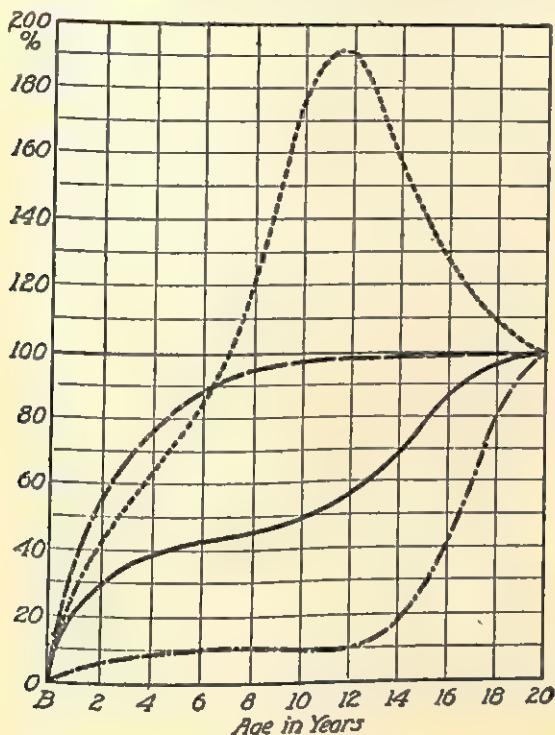
General

I think anybody who has studied the growth curves of infants and children must be struck by the fact that the whole affair is quite extraordinarily regular. Different external dimensions and different organs grow at different rates, because the head end of the foetus develops in general earlier than the tail end; thus the head after birth grows slowly, the legs, less advanced, grow quickly. But as far as is known each dimension follows a perfectly regular rate-of-growth curve with no breaks or spurts as long as the environment is optimal. The one possible exception to this statement is the mid-growth spurt, an acceleration which *may* occur and be confined to breadth and width dimensions between about five and a half and seven and a half years. The evidence for its existence is dubious, and the subject needs re-studying on more exact longitudinal growth data. At adolescence a striking growth spurt occurs in all external dimensions except fat. Immediately prior to this adolescent spurt there is a wave of fat increase, followed during the spurt by fat loss.

All this is even true of the growth of single individuals, which after all is almost more than one could demand. There are bound to be variations in any process, yet they are not sufficiently large, short of

FIG. 1
GRAPH SHOWING CHIEF TYPES OF POSTNATAL GROWTH OF
VARIOUS PARTS AND ORGANS OF THE BODY

(From Scammon, 1930, *The Measurement of Man*, Univ. Minnesota Press)



Curves drawn to same scale by plotting as percentage of adult (20-year-old) values
at successive ages

LYMPHOID TYPE

Thymus, Lymph-nodes, Intestinal lymphoid masses.

NEURAL TYPE

*Brain and its parts, Dura, Spinal cord,
Optic apparatus, many head dimensions.*

GENERAL TYPE

*Body as a whole, External dimensions
(with exception of head and neck),
Respiratory and digestive organs,
Kidneys, Aorta and pulmonary trunks,
Spleen, Musculature as a whole,
Skeleton as a whole, Blood volume.*

GENITAL TYPE

*Testis, Ovary, Epididymis, Uterine tube,
Prostate, Prostatic urethra,
Seminal vesicles.*

malnutrition and suchlike, to disturb the regular genesis of the curves that one sees. Curves of a smaller period may be imposed on the general curves. For example, there are differences between the seasons; a child grows more in height in the spring and more in weight in the autumn. This is very well established and the magnitude of the effect is considerable, but again it does not disturb the general underlying regularity of the process. I cannot help feeling there is a certain inevitability about growth which forcibly reminds one of the sort of development that, for example, Dr. Lorenz is interested in. The mechanism unwinds; it is not as though it is being pushed particularly to do so, but it just unwinds unless you get in the way and stop it.

Fig. 1 is a very famous illustration from SCAMMON (1930) that many

of you will know. It shows some growth curves from birth to age twenty. These four curves are of different tissues, to demonstrate that, though each different part of the human organism has a regular curve, these curves are not all the same. You will see that the growth of the brain and the spinal cord reach the adult level early. What Scammon calls the general type of curve characterizes the growth of most of the external dimensions such as shoulder breadth, width of the chest, height and, approximately, weight. These dimensions grow fairly fast after birth, then they slow down for a while and then have a great spurt at adolescence. There are two other forms of growth illustrated in the figure; there is the growth of the genitalia, which lie dormant until adolescence and then suddenly catch up with the rest of the organism; and there is the growth of the lymphatic tissue which is rather considerably different from the others. The lymphatic tissue grows to a supra-adult magnitude and then at about adolescence it decreases all over the body, even in organs which consist chiefly of other tissues.

The same events are shown in Fig. 2 in terms of velocity. These velocity curves, that is curves of rate of growth, seem to me often more informative than distance curves when discussing physical and physiological growth. One can see from this figure that after birth one grows more and more slowly. The brain practically stops growing in magnitude by four or five years old, for example. The growth of the external dimensions—body weight is plotted here, but it could equally well be stature, or some other measurement—decreases also in the same way, but then has the adolescent spurt. The weight of the testes shows a much greater adolescent spurt. Lastly, the thymus and the lymphatic tissues after their initial decrease actually have a negative velocity—as we have seen in the previous figure.

In Fig. 3 are the growth curves of the hip width of a group of girls studied more or less longitudinally. Above is the distance curve, plotting the average width of the hips each year; below is the velocity curve, showing the rate of growth in hip width. The time of fastest growth for a dimension like hip width is before birth, at about six to seven intra-uterine months; after this the velocity decreases steadily except that the decrease is interrupted twice. There *may* be an increase of velocity at about five and a half to seven and a half years, the 'mid-growth' spurt (TANNER, 1947). Then there is the adolescent spurt about whose existence there is no question whatever.

There is one other point worth mentioning before I leave these figures; the mechanism of form change. We do not look exactly the same as we did when babies, and this form change is due to some parts growing faster than others at various times. For example, supposing one believes in the mid-growth spurt; there does seem to be a

FIG. 2
GROWTH CURVES OF FIG. 1. PLOTTED AS
VELOCITY CURVES

(From Scammon, 1930, *The Measurement of Man*,
Univ. Minnesota Press)

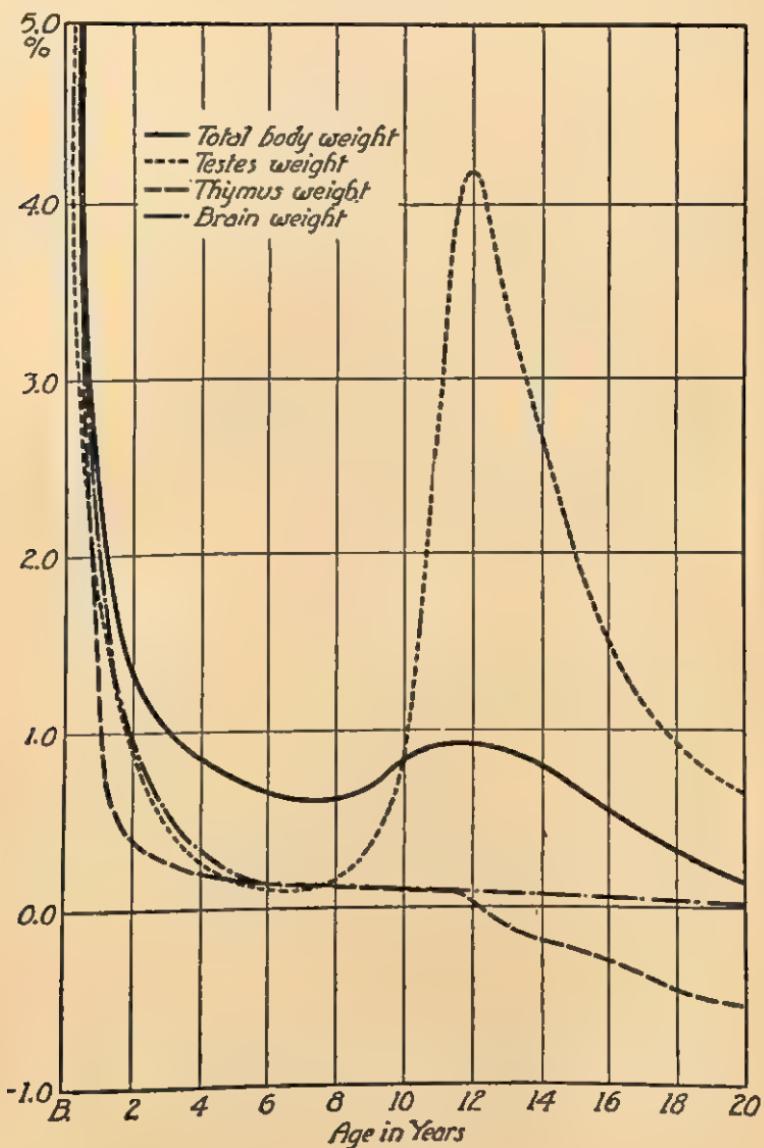
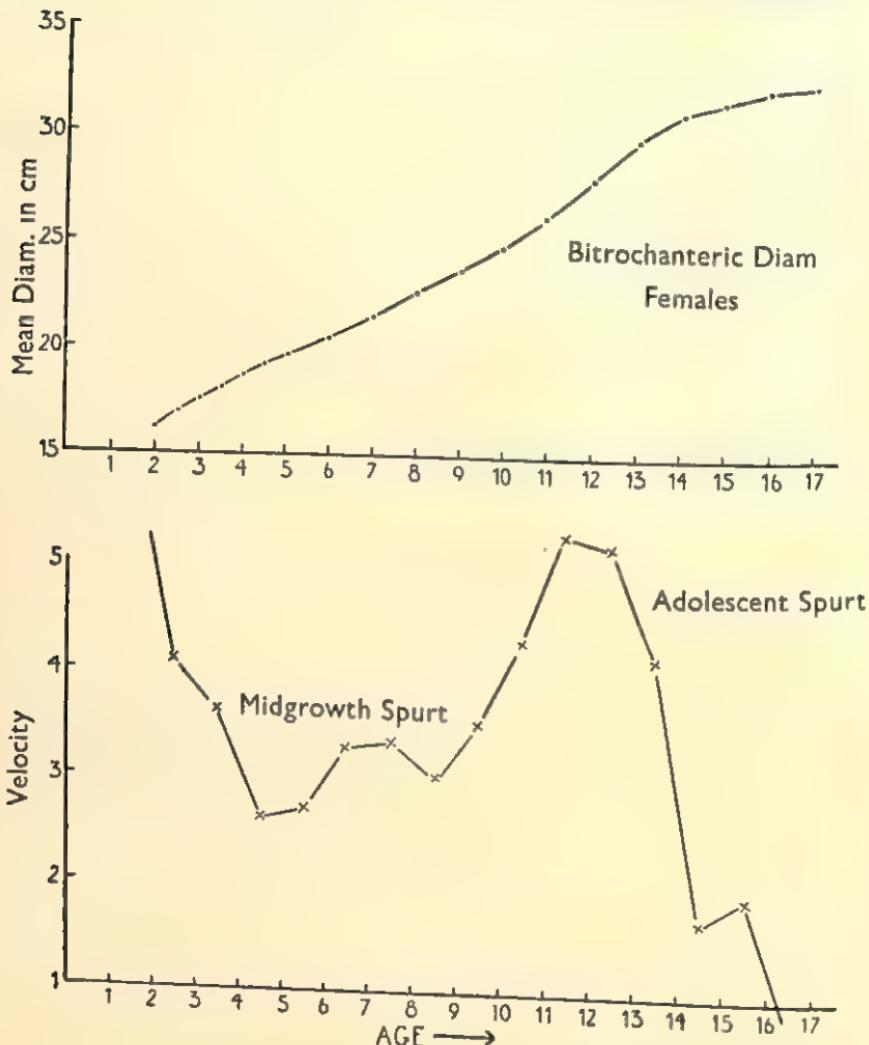


FIG. 3

GROWTH CURVES OF HIP WIDTH IN GIRLS SHOWING
DISTANCE CURVE (above) AND VELOCITY CURVE (below)

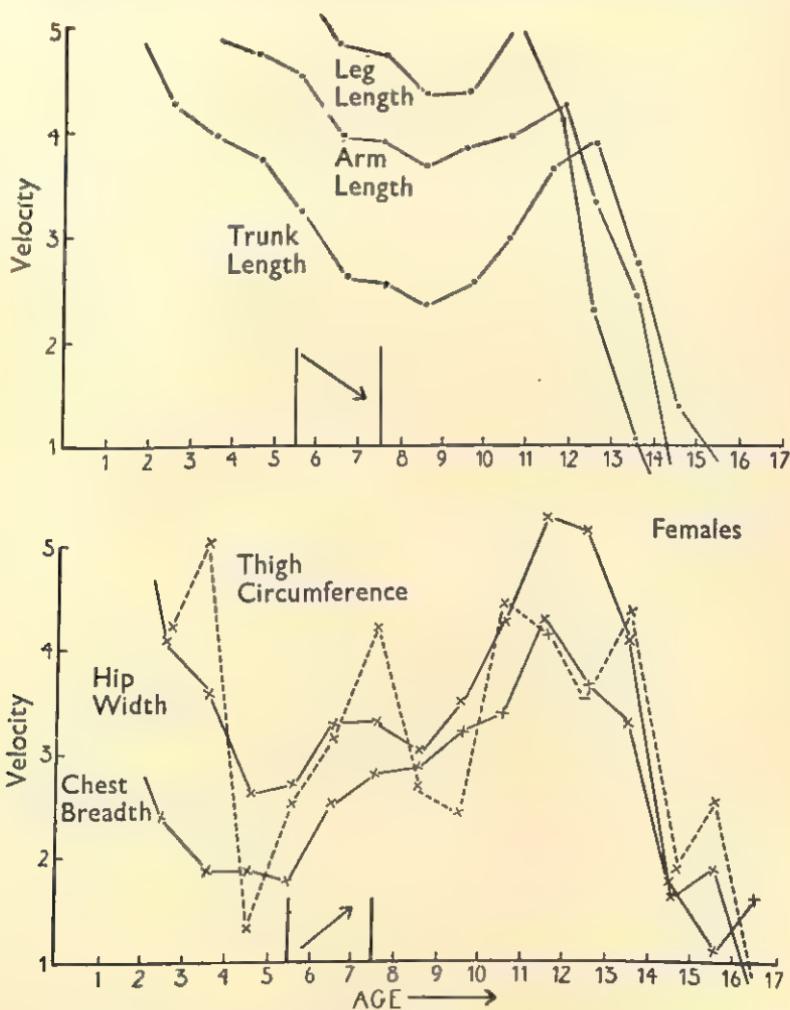


After Tanner (1947); Data from Simmons (1944)

difference between growth in length of the body at that time and growth in breadth. Fig. 4 illustrates this. It shows that the velocity of trunk length, arm length, and leg length (above) continues to fall during the mid-growth period while that of hip width, chest breadth, thigh circumference (below), increases.

FIG. 4

CONTRASTED VELOCITY CURVES OF TRUNK-LENGTH, ARM-LENGTH AND LEG-LENGTH (above) AND HIP-WIDTH, CHEST-BREADTH AND THIGH-CIRCUMFERENCE (below) TO SHOW DIFFERENCE DURING MID-GROWTH PERIOD



After Tanner (1947); Data from Simmons (1944) and Boynton (1936)

Nervous System

A few words now about the development of the nervous system. We know very little about this. We lack even the simplest anatomical facts about the development of the human brain, such as, for example, growth curves of the nuclei of the thalamus or hypothalamus. The external growth of the brain approaches completion earlier than any other bodily dimension, and there is little or no new nerve cell formation after birth, or even for some time before. At least this is true of the cerebral cortex, which is the only part about whose growth we have any good information. Very little indeed is known of the growth of the subcortical structures; the times of appearance of the various nuclei and tracts are for the most part unknown. Presumably there is a regular sequence in their development comparable to that of the ossification centres. There is the same ignorance about the development of the sense organs.

By nine months after birth, the brain is 50 per cent of its adult weight, and by two years it is 75 per cent. At about three intra-uterine months the cells become organized into the layers of the cortex, and at about six intra-uterine months the layering and the appearance that we are familiar with in the adult is present. After that the cytoplasm of the cells grows, and the cell processes enlarge, but no new cells, or very few new cells, seem to appear. One can only suppose, of course, that after this time new tracts are beginning to function, connexions are being made, and neuroglia and blood vessels are growing. CONEL (1952) has studied the cerebral cortex from birth to six months and makes one very interesting point: the primary motor area is the most advanced of any part of the cerebral cortex, the primary sensory areas are the next most advanced, and the primary auditory and visual areas follow them. In the case of each sensation the association areas are less advanced than the primary receptive areas. The cingulate gyrus, the hippocampus, and the insula, which are concerned particularly in circuits which include subcortical structures, and as a rough generalization can be said to be concerned mainly with the emotions, are developmentally behind the sensory areas at that time.

RÉMOND :

What do you mean by 'advanced'?

TANNER :

Conel takes the following grounds. Increase in the width of the horizontal layer of the cells; decrease in number of nerve cells per millimetre; increase in size of nerve cells; increase and differentiation

in the chromophil substance and in the neurofibrils. In principle Conel's point is that any particular part of the cortex changes in several ways as time goes by; and when he says that one area is more advanced than the other, he means that the changes are further along in time.

KRAPF:

Is there no reference whatsoever to the myelinization of the fibres?

TANNER:

Yes, he takes nine points altogether as his criteria, and the ninth is myelinization, which he regards, if I understand him, as not one of the most important.

That brings us to the end, as far as I am aware, of the information on the growth of the nervous system. It does not appear to have an adolescent spurt like most parts I mentioned. The skull, as a matter of fact, does have quite a little adolescent spurt, but it is said that the brain does not—I do not see how anybody can tell, with the studies that have been done up to the present time.

Physiological Development

Now I want to go on to endocrinological and biochemical development. There is remarkably little to say, once again. Indeed of all our lacks in the study of the growing child, this is perhaps the greatest. There are no longitudinal studies of hormone excretion or hormone blood levels, except over the span of a single year. There are a few cross-sectional studies from which the course of events is known very roughly. So far as this limited information goes, there is no striking change in hormonal or other internal environment from six months up to puberty. Some parts of the pituitary, some parts of the adrenal cortex, and the gonads do not function until puberty; other hormones seem to be secreted in proportion to the size of the growing child. Striking changes in the internal environment occur at adolescence, however.

One of the more interesting things that I can tell you, I think, is about the adrenal cortex which currently is believed to secrete at least three, and possibly more, sorts of hormones. One of these sorts, the gluco-corticoids, or 11-oxysteroids, hormones which are concerned in the response to stress, are secreted from very shortly after birth at the same intensity as in the adult, per surface area: there is less in absolute amount than in the adult, but then the child is not so big (TALBOT, WOOD, WORCESTER, CHRISTO, CAMPBELL and ZYGMUNTOWICZ, 1951).

FREMONT-SMITH:

Is that based on urinary excretions?

TANNER:

It is based on urinary excretions only.

FREMONT-SMITH:

So we are really still ignorant as to the actual rate of the formation, other than by this indirect method?

TANNER:

That is perfectly true. Although the blood studies that have been done do appear to concur with the twenty-four-hour excretion studies, one must be careful how one argues from twenty-four-hour excretions back to such things as blood levels and rates of secretion.

The neutral 17-ketosteroids, which are in general end-products of androgenic hormones produced in the adrenal cortex and testis, appear to start being excreted about nine or ten, in the male as well as in the female. Some people have referred to this, rather inelegantly, I think, as the 'adrenarche'. This is an inaccurate term because, as I said, one part of the adrenal has been going hard at it ever since birth. About other hormones we have no information whatever, and I ought to stress the fact that even the information I have given you is very insecurely based and subject to change as methods and data improve.

Now I want to consider *interrelations during development*. The functional state of a nervous centre may often depend on the internal environment of the body. For example, the nervous pattern of copulation and orgasm is complete in man by early childhood, but is not normally stimulated to action until sex hormones begin to be secreted, and lower its threshold to stimuli.

Conversely the whole train of events constituting adolescence is initiated by the hypothalamus (or by higher centres in the brain). The time of the beginning of adolescence seems to depend on the brain reaching a certain stage of maturation. Possibly the hypothalamic centre responsible is the very last part of the brain to become mature and functional. The postponement of adolescence is seen only in the primates, and seems to be an evolutionary point of some importance (TANNER, 1953).

You know, of course, that sometimes pathologically early adolescence occurs in girls and boys, and when this happens the events of

adolescence proceed all the way to spermatogenesis in the male and, if the circumstances are favourable, to pregnancy in the female. This raises another very interesting problem. The hormones of the anterior pituitary are present in the anterior pituitary from birth onwards, as far as we know; certainly they are present very early. They are not normally released until a certain time, probably controlled fundamentally by genetical factors, and proximately by the maturation of the hypothalamus. Tumours in the hypothalamus may cause release of anterior pituitary hormones, which bring about the secretion of testosterone or oestrogen, as the case may be, and the changes of puberty, including spermatogenesis and the production of ova. There are two exceedingly interesting papers about this; one is by Gesell and others (GESELL, THOMS, HARTMAN and THOMPSON, 1939) and another is hidden in a journal called the *Reports of the Royal Berkshire Hospital* (LE MARQUAND and RUSSELL, 1934). Le Marquand's case concerned a boy of two years old. He did not quite go to spermatogenesis, but he had adult-sized genitalia and had erections. He did not have any seminal emissions. Presumably there wasn't any semen to emit, but he had the sex-drive quantitatively appropriate to the adult. On the other hand, he behaved in a way which was more appropriate, both from an analytical point of view, and as far as common sense would have it, to the two-year-old child. He would attempt to rub his penis up against women's legs, meanwhile sucking his thumb. He had no idea about adult sex behaviour, but presumably he had his nervous reproductive centres stimulated by the hormone. Evidently the hormones had got out of step with his brain, and there must have been further maturation processes due to occur in the brain before he could produce adult sex behaviour.* In the Gesell case a similar divergence between mental and reproductive maturation occurred in a girl aged about four when she began to menstruate. Her brain, of course, was nearer to adult concepts. The whole business of adolescence is highly instructive, because we know much more about adolescence on the physical and physiological side than about any other period of childhood. Questions about the hormone secretion and growth at that period really can be answered now to some extent, whereas questions about the earlier periods cannot be at all.

Now lastly, I would like to draw your attention to a short list of some questions and of data required for answering them; then try finally to give you some picture of the growth process.

* This child was also said to have shown quite abnormally precocious interest in and skill at manipulating mechanical things such as farm tractors, motor cars, and motor cycles.

Some Questions

- (i) Is advancement in physical development as judged by bone maturity and other means related to the age at which children reach the psychological milestones?
- (ii) Hormonal and internal environments differ fairly consistently from one child to another; is early psychological development related to these differences?
- (iii) Endomorphic children appear to have earlier adolescence than endopenic ones; do they also have an earlier psychological development?
- (iv) Girls are ahead of boys in physical development up to puberty. Are they ahead also in psychological development? Do both effects disappear when (iii) is taken into account?
- (v) What is the relation between finalization of E.E.G. pattern and time at which the adolescent growth spurt starts?

Some Data Required

To settle some of these and numerous other questions we therefore need:

- (a) descriptions of the growth of the brain in the human. Such studies should give cell counts of the various parts and nuclei, allowing inferences as to when anatomical development of each ceases. They should also give details of myelinization and of any other histochemical criteria allowing inferences as to the state of functional development;
- (b) longitudinal studies of endocrine and biochemical development in association with highly accurate measurement of physical growth in a group of children;
- (c) both (a) and (b) in apes, with facilities for experimental alterations of both brain function and internal environment.

Summary

The growth process seems to me best visualized as a series of waves of activity. There is a fundamental ground plan to this which consists of a steadily decreasing velocity of growth from early intra-uterine life onwards, and on it is superimposed at least one wave of increased velocity at adolescence, and possibly another. The waves of growth do not hit each part at the same time. First the brain is growing faster and then the chest is growing faster and then the length of the legs is growing faster. We can see the flowing of that wave of activation, or whatever one likes to call it, very well in the sequence of the epiphyseal unions. This is a very constant sequence, and it is not much affected by whether everything is occurring relatively early or

relatively late. Girls are ahead of boys in physical development at birth and they remain ahead all the way through. Their epiphyses join in the same sequence as in the boys but always earlier, and their adolescence is about two years earlier.

The only thing to which I can liken this whole process is a series of clocks which are wound up and gradually run down. Now, of course, you can interfere with the running down of them by sticking your fingers in the way, which is what happens if you malnourish the child. But let us forget about that for the moment and get to the fundamental things, presuming that everything is all right for the child. The clocks run down one after the other and as one runs down it seems to initiate the running down of the next one, and so on. Consider adolescence—something in the mid-brain reaches a particular point of development, and at that point impulses go for the first time to the pituitary, and the next clock, the pubescent endocrine one, begins at this time to run down and drags the child through that phase. When we think of phases of development in this way it seems to me quite irrelevant to talk about critical phases as though there are times when an organism is particularly susceptible or particularly in danger of some influence. I do not think there is at present any evidence in physical or physiological growth for sharply defined critical periods in man, but there is, I think, a probability that an outside influence can make itself felt only between two points of time in the growth pattern. Searching my mind for an example of this, I can only find a very bad one. It is a natural one, but you will see why it is bad. If you give follicle-stimulating hormone of the anterior pituitary to a child before a certain stage of intra-uterine development it has no effect, because the ovaries cannot secrete oestrogen. If you give it after the menopause (that is, after rather a long period of growth has gone by), it has no effect either. If you give it in between, then it produces oestrogen secretion and all the things that go with that, including, of course, behaviour changes. One can conceive of susceptible periods like this occurring in the growth of the child very easily. I don't think, in fact, that there is any evidence that such things are there, but that may well be merely because we haven't got the evidence. Dr. Lorenz's experiments on mother-following in the goose and others are certainly evidence that at particular times certain neurons (if I may change languages) are capable of having their usual connexions made in a different way, though, as far as I am aware, no cast-iron evidence on the physical and physiological side exists about this.

That is all I want to say to start with. May I repeat that I feel that probably my best use in this group is to answer such questions as you wish to put on basic data from the physical side.

LORENZ :

I should like to ask a question about critical periods, and to give an example how it is in birds. There is something like the critical period, something like the finger that is stuck into the clock which is running down. There are periods when certain organs are growing fast and must grow fast, and evidently cannot stop growing. If you take a young goose in the middle stage of growth and malnourish it, it just grows much more slowly; then if you feed it up again it simply grows fast again and develops into a normal bird. Now in water-fowl, the wings grow quite slowly, nearly not at all, until shortly before fledging, and then suddenly there is a tremendous spurt, they catch up with the rest of the bird. Now if, during that period, you subject the animal to malnutrition, then the wing goes on growing as if nothing had happened to the bird, the wing is hardly affected, but the bird gets thinner and thinner and usually gets tuberculosis and dies. I would like to ask if there is something similar in the case of children, for example, towards puberty.

TANNER :

I would rather doubt that. I think the evidence is at least very equivocal. I am most interested by what you have to say, and I would be even more so to discover by what mechanism, I suppose endocrine, the bird gets its nitrogen and so forth out of itself and into its wings. Of your first example there is evidence in man; the child will starve and be generally retarded and then come back to normal later on when he is well nourished once again.

FREMONT-SMITH :

In pregnancy I think there is an equivalent. I think that the foetus does get the nourishment even in malnutrition, and I think also that in breast feeding to a considerable extent the milk continues at the expense of other portions of the body.

STRUTHERS :

But the foetus gets smaller.

FREMONT-SMITH :

Yes, but there is a tendency for the foetus to be preserved at the expense of the mother.

LORENZ :

I have a better example. There is some such relation between the important and the unimportant feathers on birds. For instance,

certain feathers must be kept for one year and some others are shed within a few weeks after fledging. If malnutrition causes a shortage of horn-producing substance the horn that is available goes into the primaries and the primaries are the last feathers to be affected by malnutrition. Some other feathers may be so dystrophied as to be hardly present at all. I have some sheldrakes at home that had to economize on horn substance, but in all of them the primaries are beautiful. Cold-blooded vertebrates can grow as slowly as they please. They do not mind malnutrition at all. A newt can catch up at any period of its life; when you start feeding it again it will grow. The bird cannot do this.

GREY WALTER:

I want to put the general zoological proposition that those characters which are phylogenetically the most recently acquired are the ones which ontogenetically are the most favoured in times of stress or emergency. As Dr. Tanner told us, the data on brain growth are extremely sparse and ill-favoured; but in the few measurements that have been made of the weight, sizes, and general proportions of different parts of the brain when people have been subjected to malnutrition, inanition, and stress, such as in the concentration camps in Germany, those parts of the brain which have retained most of their normal structure were those which developed last: that is the association, temporal, and frontal regions, while the primary motor and sensory regions tended to be relatively under-nourished and dystrophic (WULFF, personal communication).

FREMONT-SMITH:

I would like to raise a question which might underline a good deal of the discussion. I am thinking of the concept of innateness. It seems to me there is possibly an implicit idea that the environment can be passive and that the organism then just unfolds actively in it. Now I do not know whether anybody means that, but I would at least like to put into words the opposite point of view that there is a constant interaction and that all behaviour of which growth is a manifestation is a result of interaction between organism and environment with an intricate interplay in a reverberatory process, but there is no time when the environment is inoperative. Therefore, if we endeavoured to change the environment in a pertinent way, we would be able to modify any form of so-called innate or unfolding behaviour. When we do not see this it is only because the environment remains constant for the particular manifestation of growth or behaviour or unfolding.

LORENZ :

Well, I should consider that any genetically determined character is determined only in respect to a certain range of modifiability. If you take any instinctive action of a bird you can only decrease the intensity along a quite definite gradation down to zero; you can't do anything else. You would not get the slightest actual qualitative variation of his instinctive activity. If you take any more highly differentiated character, let us say, the structure of a bird's feather, its form cannot be changed as can the leaves of certain plants. I think the question of whether a character is modifiable, and to what extent it is modifiable, to what extent it is interactive with environment or whether it may be totally independent, endogenous, is a question which can only be answered by an experiment for each individual case. You cannot say out of hand that all is dependent on environment or that all is independent of environment.

FREMONT-SMITH :

The point I am trying to make is: is it on theoretical grounds tenable to assume that no conceivable change of blood chemistry, irradiation, operative procedure can change this particular character? It does seem to me hard to accept that theoretically. It seems to me that manifestations of behaviour are always interactions of the organism with the environment, because there is no such thing as organism without environment.

TANNER :

In theory, of course, no genetic mechanism can operate *in vacuo*; if the animal is completely starved, there is not much environment, and there is very soon no animal. Nevertheless, particular characters—I entirely agree with what Dr. Lorenz says—vary from being not at all dependent on the environment, except that if there is a lack of a particular amino-acid, etc., they just won't develop at all and you get a thoroughly pathological beast, to the other extreme of being genetically scarcely controlled at all. I feel that the placing in opposition of environmental and genetic features in a general sort of way is quite misconceived, because each problem is a quantitative and a particular one, and is neither qualitative nor general in any sense.

This, I think, brings us to a matter of importance for future research. The question arises as to how much the human skeleton, for example, can be modified during growth by various environmental factors. We don't know the answer to this, and as Dr. Lorenz has implied, and as I fully agree, it will not be the same for all parts of the skeleton. About the only thing that we do know is that you can retard puberty.

At the behavioural level this discussion leads on to things which seem to me intensely important, because, if I understand Dr. Bowlby rightly, what he says about the effects on children who were separated from their mothers at particular periods of growth is that environmental factors have irreversibly changed behaviour. In neurological language this implies that environmental factors have produced altered formations of neurological connexions in the brain and/or syntheses of different proteins in the neurons. To my mind one—though by no means the only—way in which Dr. Bowlby's thesis could be validated, would be by experiments to determine that these physical alterations have in fact occurred. There we have got to rely, I suppose, on the electroencephalographers, because there is nothing much else we can do with the brain except take electric currents off it. You can investigate this with animals, though.

KRAPF:

I wanted to bring up another point and to link it up with one that Dr. Fremont-Smith has made. I do not feel quite happy about some of the implications of Conel's ideas on the cerebral growth of the brain. I am referring to the remark that the first centres to develop are the primary motor, primary sensory, primary visual, and primary auditory, and that the cingulum and those structures generally referred to as emotional develop later. It seems to me that we ought to make a distinction between what we might call morphological growth and functional growth, especially with reference to myelinization, because of course it is quite true, cytologically speaking, those centres are the first to develop, but from the point of view of myelinization of pathways quite different centres have precedence. The vestibular structures, for example, are fully myelinized at the age of four intra-uterine months (MINKOWSKI, 1924, 1925). From the point of view of myelinization (see KRAPF, 1950) not only the vestibular but also the cingulum and the olfactory structures seem to develop before those others. It would seem that the functional growth as witnessed by myelinization has primary importance, and this is where I think the subject links up with Dr. Fremont-Smith's point, because according to my information it seems that the use of structures is conducive to a higher speed in myelinization, which would, of course, mean that environmental pressure, which causes certain structures to be used at a higher rate, would influence maturation.

TANNER:

I am very interested in what you have to say. As I understand you, the use of a tract causes myelinization or accelerates myelinization. I

would very much like to know what the evidence for this is, and whether the neurologists here think it is true or not.

GREY WALTER:

No, this is not true. There is no evidence that the use of an organ, or of the nervous system, accelerates growth.

HARGREAVES:

Can you retard it by the deprivation of experience?

GREY WALTER:

Nothing is known about that, as far as I'm aware.

LORENZ:

I think that there is a behaviouristic parallel though. GROHMAN (1939), of Vienna, assessed by a very nice quantitative method the maturation curve of flight. He did this by putting standard perches at definite distances from the pigeon-cote, and then he standardized the distance the young pigeon would fly after fledging. Then he got a curve for the distance at stated times after fledging. He took always two pigeons of one brood, and put them in longitudinal boxes where they could not open their wings (this is not anti-genetical, because the pigeon is a cave breeder, and breeds in cavities too small for the young birds to get exercise). Now one bird of a brood was put in a separate box, the other was left free, and then the boxed bird was liberated two days after the fledging of the normal one, and so on. Now the result was this: the imprisoned bird jumped with a bounce up to the normal curve and then went on following the normal curve. This happened to the extent that the bird which was liberated after its brother had reached the end-point of the curve, which is circling in the air, came out of the box and circled immediately, and a bird kept captive much longer got into a frenzy of flying after being liberated and then had such a hangover that it wounded its wings and could not fly for weeks. It had muscular atrophy, and it is very interesting that muscular atrophy always sets in at a time when a bird would normally move its wings. A bird does not get inactivity atrophy as long as it is in the nest. Then Grohmann did the opposite experiment; he took the control bird to its perch and made it beat its wings. He took it out, put it on his finger, quickly lowered the perch—the bird would flutter. He subjected these experimental birds to a standard time of fluttering every day. The result was that their normal flight development was retarded in proportion to the forced flying movement they

did. From this I do not conclude that such a thing as acceleration of maturation is impossible. I am quite sure that it will be possible. But I think this story illustrates what Dr. Grey Walter emphasized before.

KRAPF:

There might, of course, be some difference between what is possible in already fairly developed individuals and what is possible in a still quite undeveloped individual. In this context, I would like to refer again to MINKOWSKI's work (1924, 1925). According to his studies there is apparently a relationship at least in the foetus between use and rate of myelinization, but I am quite prepared to accept that later on this does not take place at the same rate and quite possibly does not take place at all.

TANNER:

Even for the muscular system, which one might suppose would be an easier system to study, there is little real evidence that exercise will cause anything but a temporary hypertrophy of muscle fibres which disappears again once the exercise is stopped (TANNER, 1952). That is certainly true in the adult, and nobody knows, as far as I am aware, whether it is any different in the child, because nobody has ever done any experiments which stand up to criticism in this regard. It should certainly not be uncritically assumed that taking a child and making it do exercises for a while is going to have any effect at all, except to make the fibres somewhat bigger just while the exercising continues.

MEAD:

Dr. Tanner, I was surprised that you did not say anything to us about the individual differences in constitutional types. In connexion with this critical period, is it not possible that periods that are vulnerable may be points of refreshment with slowing down or speeding up of a variety of these systems which are growing at different rates? If we have material on children of different constitutional types and we could extrapolate points in the pattern of differential growth curves, then we might get a more abstract statement of criticalness.

TANNER:

I hope you will raise that point later on because I should like later to say something about constitutional differences. There is also

one point of importance about a difficulty which fundamentally perplexes the whole field, at least on the physical side. We study the height of the child—I have shown you some curves; they are very regular. We study parts of the height measurement and find that at one stage the neck is growing very fast, at another the middle of the trunk is growing very fast, a little later the legs are growing very fast, and so on. Now bring it down to smaller elements. These, of course, we cannot study, so that we cannot really determine whether our regular curves are made up of sudden tiny, localized spurts. As far as the curve of stature is concerned I doubt whether it is, but if we think of the growth of the brain, in the foetus or at other times, then we get a little bit nearer the point, because it may be there that you get a little bit growing fast and all the rest staying still. I am not saying this does happen, but if it did we could not at present detect it.

The other thing that perplexes the field is that for the development of the brain we must have longitudinal data because of differences between individuals. Now you cannot have longitudinal data on the histo-chemistry of the brain, because once you have done the histo-chemistry of the brain of a child, the child no longer exists, so what are we going to do? In animals we can create to some extent identical individuals by breeding isogenic strains of animals and bringing them all up in the same environment. The only trouble with this approach is that the processes that are beautifully demonstrated in some species may not be present in others, particularly man. For example, the whole sequence and timing and effects of puberty in the human is vastly different from puberty in rats. The chimpanzee is two-thirds of the way to the human from the rat in this regard, and the only thing, it seems to me, is to get hold of a lot of chimpanzees to get as near the human as possible, and I know this is very expensive.

GREY WALTER :

I was going to ask Dr. Tanner exactly this question about the scale of measurements: what is the scale on which one plots results? Clearly, if you look at the mitotic figures in the individual cells, you see ample and classical evidence of the abruptness of growth. The cell remains very much the same for a long time and suddenly, in the space of hours, the mitotic spindles part and the whole thing almost explodes into two cells, so that, at some scale, one does find very sudden changes and abrupt development of functions. In the case of a relatively simple process, such as mitotic division in the normal growth of the bodily cells, one would not expect to find anything very abrupt, because the mechanism is statistically homogeneous. In other

words, you have a large number of muscle, bone, skin, and so on, cells, all doing very much the same sort of thing and distributed in homogeneous physical patterns. When, however, you come to study mechanisms of adaptation and behaviour, such as Dr. Lorenz studied and such as we study in the brain, there you are studying a relatively intricate mechanism made up of non-homogeneous statistical components, and in such assemblies you do find evidence of abrupt change; even a motor-car either does go or does not go, although the different parts of it may be statistically smooth or stationary. I think this question of the scale on which things are studied is a very important one for us to consider.

BOWLBY:

I would like to ask a very elementary question about the development of the central nervous system. Is it probable that on the whole the lower centres develop before the higher centres—for instance, is it probable that the mid-brain is functional before the cortex, or don't we know even that?

TANNER:

I think we don't know, and I think probably, if you will excuse my saying so, you are lumping together too much when you ask the question that way. Some parts of the mid-brain must function quite early but, as I said before, the part of the mid-brain somewhere near the tuber cinereum which functions to kick off the adolescence mechanism doesn't mature till very late. I want to ask Dr. Grey Walter, is this part of the brain the last one of all to mature? It would be very interesting if what happened was that human reproduction was held up until the brain was really completely matured. The question arises as to why we should have this very late puberty anyway. I presume the evolutionary answer is that before we breed we want to be as bright as possible in order to ensure the survival of the child.

GREY WALTER:

I quite agree with you that the idea of 'levels' of mid-brain and so on is really much too ingenuous.

LORENZ:

I think that the work of W. R. Hess is extremely interesting in regard to what happens in the mid-brain. We know from Hess's

experiments that very persistent and highly differentiated motor mechanisms, behaviour patterns, are localized there. These may be activated by hormones dependent on gland maturation and at the same time be dependent on the nervous system. You can induce mother behaviour in a common barnyard cock by injecting it with prolactin because he has the nervous mechanism for brooding young. But you cannot do it in the White Leghorn, because in the central nervous system of the White Leghorn brooding has evidently suffered a defect mutation; brooding cannot be produced either in the hen or in the cock. A similar question is interesting in regard to the behaviour of your true pubertas praecox, and I should be very interested in having details of their behaviour, whether they were girl-conscious, whether they courted beautiful girls, and so on.

TANNER :

I am afraid I cannot answer that very well. It is about a couple of years since I read one of the papers. All I can remember is that the little boy of two years old showed a marked preference for some women over others.

LORENZ :

Were they pretty?

TANNER :

No, it was a more basic phenomenon, if you will pardon my calling it so. It was said in this paper that he did not pursue women who had passed the menopause, but he did become very difficult in the street because if he saw someone under forty in a nice pair of silk stockings he would make a dash for them and rub himself against them, and this got quite embarrassing. But he did not go at all for young, prepubescent girls. Neither did he ever make sexual advances towards his mother. This patient was in the Royal Berkshire Hospital, and I may add that he was studied only by ordinary doctors with both the advantages and disadvantages that that implies. There were no psychologists around, so that the study is not influenced by preconceptions of a psychological nature.

GREY WALTER :

We had a prepubertal case, a child of six who showed very good taste with the nurses. He knew the pretty nurses extremely well.

TANNER :

Well, that shows the difference between two and six, which is just what we are after.

ZAZZO :

I should like to put three questions to Dr. Tanner. The first is as follows: is the present state of our knowledge of allometry and of non-parallel growth rates sufficient to enable us to understand the critical phases and stages of growth? In other words, despite the fact that there is a slow, continuous progression, as Dr. Tanner reminded us, are there for the human species rearrangements in the balance of the constituents and allometries which enable us to understand or at least to guess the explanation of the stages of growth, and to describe the critical phases? For us psychologists it would be important to see if any co-ordination is possible between critical phases found by developmental biology and critical phases which we observe in psychology.

My second question is much more limited. What is your opinion of the frequent statement that the age of sexual maturity in girls is earlier than in boys, since the appearance of menstruation is earlier than that of spermatozoa? Can we consider that as an established fact? Much research over the last forty years has tended to prove the contrary. Is this current belief not due simply to the fact that girls start on the phase of accelerated growth before boys? This is important for the understanding of adolescence on the psychological level.

My third question deals with the lowering of the age of puberty, which seems to have been occurring during the last fifty years, that is since it has been possible to obtain reliable data. Is the variation in age of puberty with different social environments considered by Dr. Tanner to be a well established fact, explainable by nutritional factors? In other words, what connexion can we establish, in a general way, between the evolution of the organism and environmental influences, 'environment' being very widely interpreted?

TANNER :

I will take the questions in the order in which they were asked. As I understand it, your first question implied that the study of allometry in the physical side of development gives some background for the suggestion that there are critical phases during growth. When allometry was first thought of by Huxley and Teissier and others, they imagined that by plotting the growth of one dimension against another, both in logarithms, they produced a curve with a sudden

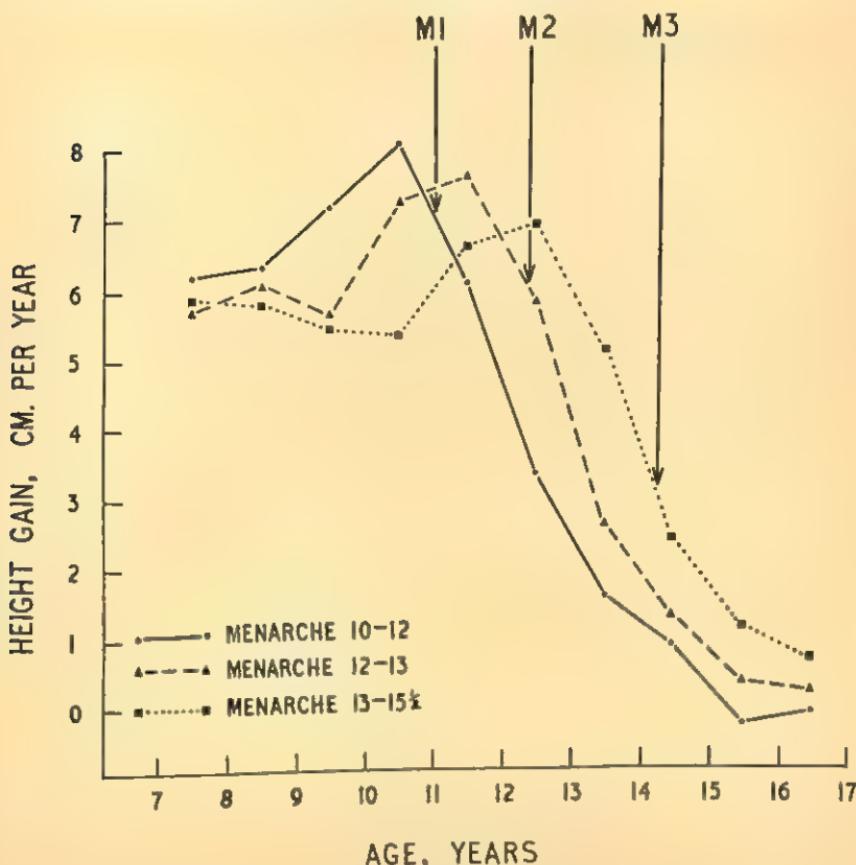
sharp change of direction in it. Later work has shown this notion to be quite fallacious, at least for mammals. The sharp change of direction resulted only from drawing various straight lines through points which really show a gradual and continuous curvature (TANNER, 1951). Any idea that critical phases may exist because of that formulation of allometry is ill-founded and incorrect. That answers the question without becoming technical, which I am very willing to do if we have the time.

Now the second question—the difference in age of sexual maturity between girls and boys—first, is it established that girls begin puberty earlier than boys? Yes. Definitely, clearly, no question. About two years on average. It varies between individuals of course. There are some girls who enter puberty after some boys. There are some interesting data that suggests that those girls who develop late become relatively more masculine in physique and those boys who develop early become relatively more feminine (BAYLEY, 1943). I don't think this is entirely established, but it is reasonable, and it is a subject which would stand looking into. So much for the spurt in physical growth. However, if I understood Dr. Zazzo correctly, he is more concerned with the attainment of actual reproductive maturity in the strict biological sense, that is, spermatogenesis, copulatory behaviour, the ability to produce children. The evidence here is not so complete. I *would* imagine girls are capable of breeding earlier than boys: on the other hand, there is a well-documented period of adolescent sterility. The course of puberty is this: there is an increased velocity of growth in stature and in all the physical dimensions, and shortly after the peak velocity has been reached and the speed is slackening to zero, menstruation begins (see Fig. 5). This relationship is remarkably constant. The uterus grows fairly late and gradually becomes functional, and as growth in height begins to finish, menstruation begins. Now, despite the uterus getting to the stage of bleeding, the early cycles, which are often not so regular as the later ones, may be anovulatory, and due only to oestrogen withdrawal, and this may go on for perhaps a year, eighteen months, two years. There is some physiological evidence for this and also a good deal of anthropological evidence that repeated intercourse at that time does not lead to pregnancy. It may well be that there is a similar period in boys. There is no evidence about this as far as I know, but it seems a reasonable thing to assume. That's as far as I can go towards answering your question on the facts we have at present.

Now for the third question: is the age of puberty related to environmental factors and social class? Yes, it can be related to environmental factors; for instance, adolescence was retarded in parts of Europe where the food supply was diminished by the war. There is no

FIG. 5

RELATION OF PEAK VELOCITIES IN HEIGHT-GROWTH FOR
EARLY, AVERAGE AND LATE MATURING GIRLS TO TIME OF
MENARCHE. M1, M2, AND M3 SHOW AVERAGE TIME OF
MENARCHE FOR EACH GROUP



After Tanner (1953); data from Simmons and Greulich (1943)

question that malnutrition can postpone it. I am not sure whether there is any literature which distinguishes clearly between different social classes and whether any class effect could be related to things other than malnutrition, or whether indeed it could be related to genetics. As to other environmental factors such as climate, it has often been said that puberty is earlier in the tropics. But the data on which that statement was based are very poor. There has been one good study by ELLIS (1950) recently, which showed that puberty occurred at very much the same time in Nigeria as in Western

Europe. It was shown in California that different racial groups had menarche at different times, but this may be chiefly due to genetical factors (ITO, 1942). The differences were not in any case very large. So this question cannot be answered certainly, but short of malnutrition it seems that if there is any social class effect, it isn't a big one.

MEAD :

Would you include the work that PELLER (1940) did on the effects of starvation in the mother during pregnancy on delayed menstruation in the daughters?

TANNER :

I don't know that work; I would like to have the reference.

MEAD :

These studies were based on Viennese girls whose mothers were pregnant with them during the worst period of starvation in Vienna in World War I, and there was a consistent postponement of the age of menstruation in the daughters. The implication was that in handling first-generation data on ethnic groups we perhaps ought to go back a generation in order to trace the causes of any differences.

TANNER :

Or rather to the foetus. I regard this with a certain amount of doubtfulness because most of the data say that semi-starved foetuses such as we are assuming to exist would only be small and perhaps developmentally retarded at birth. Most of the data (though these data are not cast-iron stuff at all) would rather indicate that if the children were later well nourished they would catch up on their growth curves rapidly, and certainly not be thrown back all that long time during growth, as you imply.

MEAD :

There were other concomitants also. There were pelvic distortions, sometimes prolonged amenorrhoea after puberty, and difficulty in childbirth.

CAROTHERS :

It has been commonly written and commonly assumed in the past that puberty was early in the tropics, but this was largely based on estimates of age. The African never knows his age and these estimates

were grossly faulty. They are not quite so faulty at the age of puberty but at later ages it is easy to be ten years out, and the age is nearly always under-estimated.

KRAPF:

With regard to the matter of the onset of menarche in South America, what I have been able to find out from gynaecologists' reports about the matter is against any differences between the temperate zones and the tropical zones.

SECOND DISCUSSION

The Behaviour of New-born Anencephalics with various Degrees of Anencephaly

MONNIER:

We have made a prolonged study of the scheme of integration of the motor functions by the central nervous system. Various methods can be used. The best way is to study the development of the motor paths in the embryo, foetus, and newborn, a method initiated by Monakov and Minkowski at Zürich and experimentally perfected by Windle at Chicago. Phylogeny and ontogeny supply us with concordant data which enable us to reconstitute the scheme of integration (*Aufbauplan*) of the motor functions provided that these data are analysed and interpreted according to functional criteria (MONNIER, 1946). If we admit as a criterion of integration the synthesis of the elementary mechanisms in a function adapted to an aim, we can say that the scheme of integration advances by stages at well defined times. We have distinguished the following stages:

1. Integration of motor functions in respiration and nutrition, functions of mime and vocal expression, protective functions with predominance of flexion mechanisms and functions of prehension (end of foetal life, and birth);
2. Functions of active orientation of head and eyes (two to three months);
3. Functions of lifting the head (two to three months), the trunk (five to six months), the legs, retaining position (seven to ten months);
4. Functions of progression, locomotion, and regulation of equilibrium (eleven to fourteen months);
5. Articulate language (fifteen to twenty-four months);
6. Technical manual dexterity characteristic of working man (adolescence).

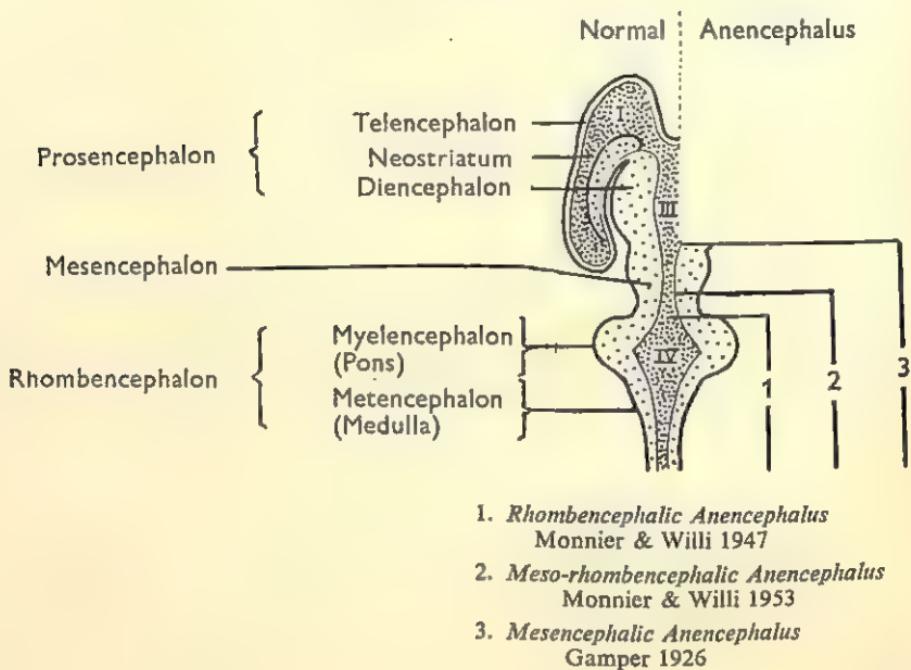
Although it is always possible to study experimentally in the animal the correlations between the stages of development of motor functions and the stages of differentiation of anatomical functions, which give

us information on the integrating function of the differentiated mechanisms, the same cannot be done with man. This is why the newborn anencephalics, with their rudimentary bulbo-spinal, ponto-bulbo-spinal or meso-ponto-bulbo-spinal brains afford us an exceptional opportunity of defining the correlation between a stage in the organization of motor functions and the corresponding stage in the morphological development of the nervous system. The film will show the motor paths of ponto-bulbo-spinal anencephalics (rhombencephalic anencephaly), then those of a meso-rhombencephalic anencephalic and finally those of Gamper's anencephalic with a well developed mesencephalon. At the same time anatomical sections of the brain stem will be shown, illustrating the degree of development of the nervous integrating mechanisms.

(a) *Rhombencephalic Anencephalus*

We have been able to observe four anencephalics whose brain was limited to the pons, medulla oblongata, and cord. In such cases the vegetative functions are very poorly regulated. The blood supply to the peripheral area is insufficient and respiration is irregular. Ingestion of food is badly co-ordinated, especially sucking. The temperature is very labile (poikilothermy) and the infant can hardly survive more than one or two days. There was no spontaneous activity and no periodicity of states of wakefulness and sleep. Motor activity consists mainly of defence behaviour by flexion with intense protective reflexes to noxious tactile stimulation or to acoustic or vestibular stimulation. Integration at this stage is characterized by poor localization of reflex responses, a tendency to irradiation, bilateralism, and even generalization of response (mass reflex). Stimulation of the sole of the foot, the malleolus, or the Achilles tendon produces, not a Babinski phenomenon confined to the big toe, but a triple retraction of the leg on the side stimulated, often also of the other side, and sometimes even a reaction of the upper limbs accompanied by clonic trembling of hands and arms. Reflexes of flexion posture and prehension reactions are highly developed. Rudimentary balancing reactions are observed with extension of the arms and flexion of the head, as in Moro's reflex. Although conditions for development of mental activity do not perhaps exist it can at least be said that mechanisms for protection against noxious stimuli and prehension to counteract the dangerous effect of weight are accompanied by expression phenomena capable of interpreting certain rudimentary affective states. The mechanisms responsible for these primordial integrations are the reticular formations of the medulla oblongata and the pons (Tegmentum pontis), with the posterior cords and the cranial nerves V to XII (MONNIER and WILLI, 1947).

FIG. 6
ANENCEPHALIC NEWBORNS AT VARIOUS STAGES OF ANENCEPHALY



(b) *Meso-rhombencephalic Anencephalic*

I had the opportunity of observing with H. Willi an anencephalic whose brain was limited to the caudal area of the mesencephalon (isthmus) together with the pons, medulla oblongata, and cord. It was possible to keep the newborn alive for fifty-seven days. It showed good circulation, irregular and sometimes periodic respiration, poikilothermy and mediocre regulation of blood sugar. Spontaneous activity was limited to a few movements of the face and lips. The infant lay on one side with arms and legs flexed. It had the advantage over the type previously described of having a more physiological posture during sleep. The hands were prone and legs supine. It slept most of the time and showed little movement—only lazily or by fits and starts—when stimulated. The head sometimes moved slowly from one side to the other.

The somatic-motor integrated functions consisted mainly of defensive flexion reflexes released by exteroceptive tactile, thermic or chemical stimuli, more or less noxious. Very intense stimuli provoked a generalized defence reflex (mass reflex). Grasping mechanisms were well developed in both hands and feet; they could be clearly observed

during trophotropic activities, such as sleep and feeding. Functions of mime and vocal expression were well developed: contortions of the trunk, vermicular movements of the head and limbs, sometimes certain facial expressions of affective pleasure reactions with stretching, extension of the arms, yawning and sighing. Passive extension of the leg induced, for example, a Moro reflex, with deflection of the head, extension of the arms, especially the right arm, opening of the mouth, and sighing. As to vocal reactions, they were limited to a sort of hiccup and stridor. The functions of extension and of lifting against weight remained rudimentary: reactions of extension and abduction of the arms (stage 1 of Moro's reflex) with lifting of the trunk. As regards functions of active orientation of the head in space, they were limited to mechanisms of buccal prehension (prehension of fingers or bottle with lips, lifting of the head under the influence of proprioceptive excitation from the stretched nape of the neck or from the vestibular apparatus). The lifting of the trunk in sitting position by passive extension of the legs can also be considered an elementary lifting mechanism.

In this type of anencephalic we were able to observe in the course of weeks the transformation of an inadequate reaction (turning of the head towards a noxious stimulus: prick of a needle) into an adequate flight reaction. This adequate reaction appeared seven weeks after birth; it was at times so intense that it even took the place of buccal prehension behaviour; the mouth moved away from the finger presented instead of seizing it (MONNIER and WILLI, 1953).

(c) *Mesencephalic Anencephalic*

In the anencephalic of pure mesencephalic type, described by GAMPER (1926), all the vegetative functions are well regulated, especially respiration and circulation. There is a distinct alternation between periods of sleep and waking activity. General spontaneous activity is more intense (movements of the tongue, contortions). Locomotor paths are highly developed; they consist mainly of reflexes of crossed extension, automatically released, and very clear successive induction phenomena in sitting position (epileptoid trembling). The functions of facial and vocal expression are better elaborated: smiles and tears. The Moro reaction, induced by a puff of cold air, is shown again by extension of the arms, but also by deflexion of the head, with a yawning reaction, whereas in the rhombencephalic anencephalics we had mostly a head-flexion reaction. The functions of protection and defence are well developed. Extension and lifting functions are definitely better developed than in the preceding anencephalic types. The same is true of the tonic neck reflexes, the reflexes of lifting of head and trunk, and the reactions to vestibular

stimulation of rotatory or linear acceleration. This is also the case with all the functions of orientation of the head and upper body in space. The paths of oral prehension are so well developed that they produce in the anencephalic an effect of an automaton, which always directs its mouth towards the finger that touches it. Here too one can say that the development and maturation are characterized by greater spontaneous activity, mainly pre-locomotor, by better differentiated functions of facial expression, giving the impression of affective reactions, better elaborated functions of lifting and motor orientation and especially by a closer and more adequate adaptation of reactions to objective (GAMPER, 1926).

Dr. Monnier then showed a film illustrating these descriptions.

MONNIER:

I would like now to discuss the question of reactive patterns changing with the development of the nervous system. My film showed a meso-rhombencephalic anencephalus turning the head toward a source of nocive stimulation (prick of a needle) during the first six weeks of life, and then turning the head away from the stimulus. This latter protective reaction could be elicited chiefly from the seventh week after birth. Ipsiversive and contraversive patterns of head turning were probably both integrated in the brain at birth, but the threshold of the two systems was different: low threshold for ipsiversive turning during the first weeks (tegmental reaction), higher threshold for contraversive turning (protective reaction). The threshold of the latter decreases only seven weeks after birth, as a sign of greater maturation of the brain.

FREMONT-SMITH:

What is the reaction in the normal infant?

MONNIER:

Always away from the stimulus.

GREY WALTER:

I beg to doubt that. There is a great difference in different children's response to stimuli in different parts of the body. If you study the same children with different strengths of stimuli you get very different responses. It is a matter of balance between the different muscles, and is very often a matter of chance, excluding the effect of well-established reflexes. It depends on the precise state of development of the motor-groups. Have you studied different strengths of stimuli?

MONNIER:

When I said: always away from the stimulus, I meant, of course, a nocive stimulus; the prick of a needle. In normal newborns we got an ipsiversive turning of the face and mouth on a slight stimulation of the lips.

I should like now to define growth and maturation as a process which locates, which circumscribes a reaction, instead of spreading it. Diffusion and generalization are expressions of an immature nervous system. During growth the reaction becomes more adequate, better adapted to its purpose. I gave as an example the meso-rhombencephalic anencephalus, which showed a better adaptation of the head reaction to various stimuli seven weeks after birth; head turning away from the nocive stimulus. In this regard, we have to consider the notion of threshold. We know in the tegmentum of the brain stem there are pre-configurated systems for ipsiversive and contraversive head turning. These systems must have at different times different thresholds. During the first six weeks, we elicited chiefly ipsiversive patterns; during the second month, as maturation of the brain progressed, the contraversive reaction of the head, away from the nocive stimulus, became more prominent. There was even a kind of rivalry between ipsiversive responses (turning of the head with sucking) and contraversive responses (flight reaction). It was sometimes even difficult to feed the baby, since the flight reaction of the head was too strong, so that the nurse could not get near the lips with the bottle. This rivalry was typical for the transition state.

What other conclusions can we draw from our observations and from the film, in relation to the problems of psychobiological development? We may say that in the lowest rhombencephalic type of anencephaly, the protective patterns of flexion type are predominant. The surface of the body is reduced in order to escape nocive stimuli. At a higher integrative level (meso-rhombencephalic type) some elementary mechanisms of standing and righting postures develop. On the other hand, the mime changes with maturation; it becomes more elaborate, sighing appears with contortions of the body, which have sometimes an expression of pleasure or displeasure. We do not know, of course, if a psychological experience occurs behind these various expressions.

GREY WALTER:

May I ask Dr. Monnier a question about the conditions during the experiments that were shown in the film? I, too, am particularly interested in the reversal of the behaviour of the child or creature from what one might call a positive to a negative tropism. This

effect, as you know, can easily be imitated in a model. It requires only a simple set of arrangements to exhibit and mimic this particular development, but the effect produced both in the flesh and in the metal depends a great deal on the experience of the individual, whether man or machine. For example, in an ordinary healthy baby one may see a reversal of attitude or of behaviour, from the positive attraction to the breast or the bottle—sucking movements—to a withdrawal when the stomach is full either of milk or of wind. I am sure most of us with children have suffered from this difficulty of trying to get enough milk into the child; at a certain point there comes the avoiding reaction which perhaps may develop in the adult into a highly organized negative shaking of the head. It would be interesting to know in your film the state of the stomach of the child during these two experiments; that is, whether you are quite sure that the child is equally well fed or has an equal amount of wind in his stomach during both experiments, because one would expect that after a certain length of time, varying in the normal child from a few weeks to a month, this reversal would occur as a result simply of the presence of a distended stomach. That is, the endogenous stimuli in the belly can reverse, in fact, the whole action of the body.

MONNIER :

I am not able to make comments on the influence of the stomach and of hunger on the responses of our anencephalic newborns.

GREY WALTER :

The difficulties of feeding are found in quite normal children. I wonder whether you had any evidence of the presence of wind in the stomach. Did you X-ray the stomach to see if there was much gas?

MONNIER :

No.

FREMONT-SMITH :

Isn't your question partly answered, Dr. Grey Walter, in the fact that the ipsiversive reaction occurred nearly every time the child was tested in the early period, and the contraversive nearly every time he was tested in the later period, and therefore it would seem surprising if there was always an excess of wind in one period and not in the other. I might add that I have seen an infant up to the second and third, maybe fourth, day having difficulty in getting breast milk, being unsuccessful, and finally exhibiting a negative reaction so that

he couldn't be got to accept the nipple. Presumably a normal child will still feel hungry, although I don't know how much air he has swallowed.

PIAGET:

At the beginning of his interesting communication Dr. Monnier said that he did not consider that growth could be determined by generalization because generalization is a primitive stage which comes before differentiation. I think there is some ambiguity, not in what Dr. Monnier said, but in the discussions in general, particularly in the explanations given by Pavlov's followers of the formation of language by conditioned reflex. Actually this term generalization can be taken in two ways, which appear to me to be very different and which must be carefully distinguished if we do not want to become involved in confusion associated with the definitions of psycho-biological terms.

The word 'generalization' can be first taken in the sense used by the reflexologists as being a level of initial non-differentiation. Generalization, in the sense of a reflex, is a level where reactions are not yet differentiated or specific. I would call this type of generalization 'automatic generalization'.

But there is a second kind of generalization which plays an important role in growth and in the development of intellectual functions, which I would call 'intentional generalization'. Contrary to automatic generalization it depends very much on differentiation; the two are in fact interdependent.

I will give an example. One of my children at about four and a half months, an age when he was beginning to take hold of objects within his visual field, in the early stages of co-ordination between vision and prehension, started to take hold of a cord hanging from the canopy of his cot. When he took hold of the cord it shook all the dolls which we had hung from the edge of the canopy and this immediately gave rise to a circular reaction, a plan of action which he kept on repeating. In this case the plan gave rise to generalizations but these were accompanied by very clear differentiation. When a new doll was hung from the canopy you could see the child looking for the cord and pulling it while looking at the doll, but if a doll was held in front of him he did not look for the cord, he had other reactions. Later on I tried the experiment of hanging an object not from the canopy but from a long rod about two yards away from the child. I made it swing. The child looked at this with great interest and smiled, but the moment the movement stopped he looked for the cord because of the hanging object. Here we have a generalization

which is a sign of development, a generalization based on differentiation and discrimination, an active and intentional generalization which should not be confused with automatic generalization.

If it is a question of automatic generalization, then I am in full agreement with Dr. Monnier, but not if it is a matter of intentional generalization.

I am insisting on this distinction because in Pavlovian discussion on the formation of language based on conditioned reflexes the great difficulty is to explain the generalization inherent in the use of words, nouns and concepts. Very often there is a change from the first meaning to the second and from the second to the first. I repeat that according to the second meaning generalization is a very clear sign of development of everything connected with cognitive functions.

KRAPF:

I wonder what one would possibly derive from considering the newborn's ipsilateral and contralateral reaction movements of the head in relation to the genesis of object relations. We have apparently a changeover from what we might call the ipsilateral reaction to the contralateral reaction, and at a certain time we even had, according to this film, some sort of conflict situation between ipsilateral and contralateral tendencies. Now I hesitate to give a psychological interpretation to the ipsilateral reaction, but I think we cannot go far wrong if we consider the contralateral reaction as a flight reaction from something unpleasant. If one considers the matter from this point of view, it would seem to me that it bears out an idea as to the genesis of the concept of object. It would seem reasonable that in the beginning the newborn has no concept of object at all; he considers the object part of himself. It seems to me that in all likelihood the genesis of the concept of object is related to the first experience of objects which are not dominated by him or belonging to him, that is to say, to introduce a term of Melanie Klein's, bad objects: objects which are opposed to the newborn and which, therefore, for the first time put him up against the necessity of considering that there are certain things in this world which are hostile and against which the better choice is a reaction of flight. I felt that Dr. Monnier's film showed very clearly not only how the ipsilateral embracing of the object expressed the all-belonging quality, but also how the contralateral reaction, the first awakening of the concept of the hostile object, arose, and how the two entered into conflict, which in all likelihood is one of the first conflicts which the newborn experiences. This I feel might be an interesting aspect of the problem of neurotic conflict later on in life which can be derived from the

purely physiological considerations and builds a bridge towards psychological and psychoanalytical concepts of this phase of development.

MONNIER :

I do not know if we are entitled to bring primitive ipsiversive patterns in relation with the psychological concept of all-belonging behaviour and the contraversive pattern with the concept of hostile rejecting behaviour. Of course, in our anencephalic newborns, the finger is not recognized as an object. It is just a source of stimulation. We must ask Professor Piaget at what time the notion of an object develops.

PIAGET :

That depends on the criterion you use for the idea of object. If you take as criterion the search for an object which has disappeared from the perceptive field; for example, if you put a watch in front of a baby and just at the moment when he is about to take hold of it you cover it with a cloth, the reaction remains negative until about six or seven months. It is only between eight and ten months that we find the search for an object which has disappeared behind a screen. Before this behaviour is organized, very interesting intermediate stages can be observed where the object is not yet localized. Thus, if you put a baby between two pillows, one on the left and one on the right, and you place a watch under the right pillow (you do this at the time when the baby is beginning to search behind a screen) the baby lifts up the right pillow and takes the watch. Then in front of his eyes you put the watch under the left pillow. I have seen infants of eight or nine months at the moment the watch disappears looking again on the right where the action was successful the first time. Therefore there is not yet permanence or localization of the object. Consequently if you take as criterion an object which is localizable outside the perceptive field only towards the end of the first year do you get search for an object that has disappeared.

INHELDER :

In the light of present experience it seems impossible to fix on an exact time, say four and a half months, for the sensori-motor co-ordination necessary for prehension of the object. There is always a certain learning in the act of taking hold of an object even when the object is within the child's visual field.

PIAGET:

This happened at different ages in my three children. In the first it was at four and half months, in the second at six months and a few days, in the third at three months and a few days. With the third I had carried out a series of experiments on imitating hand movements which seem to me to have played a part in this early reaction.

HARGREAVES:

I want to pose the question of the Spitz smiling reaction. It seems to me that is the first evidence of the perception of objects. What is Dr. Bowlby's view about that?

BOWLBY:

I think we have to distinguish between responding to a perceptual stimulus as an Innate Releasing Mechanism does, and the ability to conceive of an object persisting even when it isn't being perceived and responded to. I should have thought these were quite different things, and that Spitz was talking about the I.R.M. response principally, and that his work indicates that it isn't until after six months that an infant develops the notion of a persisting object.

ZAZZO:

I think the smiling reaction is a very complex phenomenon. The origins of imitation are still rather mysterious. Eight years ago I observed a fact which I consider almost inexplicable. I noticed in my son, who was then twenty days old, the imitation of putting out the tongue. At first I thought it was a mistake, that it was I who had imitated the child and not the child who had imitated me. I tried this experiment again several times. I took a film of it and later I was able to examine six infants in whom this imitation also appeared between the twentieth and thirtieth day, then disappeared, and reappeared at the age of about three months. I have formulated some hypotheses on this subject but they seem to me not very sound. I tell you about this fact because I should like to know if any of you have noted such early signs of imitation.

PIAGET:

Is the imitation which appears at three months lasting?

ZAZZO:

It is lasting on condition, of course, that it is cultivated. You hold the child, he is close to your face, he concentrates on your mouth.

He starts putting out his tongue when you do; he smiles when you smile; he starts again when you start again. The imitation of protrusion of the tongue then again becomes contemporaneous with the smiling response. But there is that very early imitation at the age of twenty days which is strange and which perhaps challenges our over-intellectual theories on imitation.

LORENZ:

The athetotic movements which are seen in prematures are very reminiscent of stretching movements with young. Stretching, like many other instinctive activities, has a very definite gradation of intensities and, at the highest culmination of intensity, it is always accompanied by yawning. That this very same thing occurred during the athetotic movements of your anencephalic in the film seems very important to me. Furthermore, your friend Professor Gamper, in the titles of the film, always uses a word which quite simply means stretching, so evidently he himself had no doubt about the fundamental identity of stretching and athetotic movements. In the adult, matters seem to be somewhat different. I have seen lots of patients with lesions of the pallidum and consequent athetotic movements, but the impression that there was a similarity between these movements and stretching never occurred to me. But it did instantly when I first saw the athetosis of a premature. And this impression was very strongly accentuated by your anencephalic yawning while doing athetotic movements. The beginning of it was definitely athetotic and the end was in yawning.

There is one other point on which I wanted to utter an opinion. I agree with Dr. Monnier's interpretation that two mechanisms were in conflict, that this creature actually did the 'oral prehension' reaction and at the same time the flight reaction too. I think that this interpretation is quite correct because conflicts are not in any way dependent upon the presence of higher nervous functions. You get true conflict on the level of the medullary preparation. It is the conflict between two postural automatisms. Instinctive actions or instinctive activities of any kind primarily overlap. It is not the general rule that they are mutually inhibitive. They are mutually inhibitive only in special cases where overlapping would be harmful. Flight reaction must not be overlapped by any eating reaction. If an animal running away did a mixture between running away and eating it would be very bad for him. The overlapping of sexual and aggressive reactions in fishes, which we are investigating just now, is so common that you find the greatest difficulty in ascertaining which movements are motivated by the sexual instinct and which are motivated by the aggressive

instincts. Judging by all we know, the factors which keep the several instinctive activities apart and prevent them from going off simultaneously, in a 'cacophony of movement', as Tinbergen expresses himself, or, more simply, in a fit, lie in the afferent control. There is no doubt that the greater part of motor mechanisms are localized farther to the caudal end of the brain than are the controlling afferent mechanisms. In the anencephalus, devoid of all but the most caudal parts of the brain, an uncontrollable overlap of motor activities is therefore only to be expected.

MONNIER :

I think that we must distinguish the athetosis of the trunk from the athetosis of the extremities. The latter appears chiefly when the inhibitory action of the striatum, and more precisely the action of the caudate nucleus, is abolished, as a consequence of extensive lesions. The lower centres of the brain stem deprived of the striatal control are then released and show an increased activity, in the form of athetoid movements of the face, fingers, and toes. Athetosis of the trunk develops in a similar way in the rhombencephalic anencephalus, when integrated structures of the tegmentum and of the reticular formations of the pons and medulla are released from the diencephalic control. The athetoid stretching of the spine and neck, with yawning and worm-like contortions of the body, are mechanisms which must be integrated in the tegmentum of the midbrain and pons, as well as in the reticular formation of the medulla.

As for the question of conflict between ipsiversive and contraversive patterns, I agree entirely with Dr. Lorenz. Both patterns must have at the same time a different, well-integrated anatomical substrate, with different thresholds; the excitability of each substrate may vary according to the degree of maturation and to the various endogenous conditions. A conflict situation occurs because the afferent controls are limited to spino-bulbar systems, in the rhombencephalic anencephalus, whereas in normal newborns cerebellar diencephalic and cortical controls, with their afferents, are still acting.

THIRD DISCUSSION

Criteria of the Stages of Mental Development

INHELDER :

I find myself in a most unenviable position. To begin with, I have been asked to expound Piaget's conception in front of Piaget himself. The conception of mental development as it appears in the works of M. Piaget is somewhat disconcerting, not because of the facts but because of the terminology. M. Piaget, who is a zoologist by training, an epistemologist by vocation and a logician by method, employs a terminology as yet not much used in psychology (PIAGET, 1951a, 1951b, 1952). He expresses himself mainly in terms of *structures*, which by definition are systems of mental operations obeying definite laws of composition such as, for example, the mathematical laws of group and lattice. According to a number of cyberneticists structures are as much physiological as mental. It seems to me necessary to keep in mind this triple orientation—biological, epistemological and logico-mathematical—which is continually reflected in Piaget's vocabulary, in order to find one's way easily among the Geneva studies. But once these characteristics are appreciated the data and laws deriving from them become clear and are easily verified.

The general subject of this meeting is a determination of the criteria of the stages of development. How can we define a stage of development from the psychological point of view? The schools of Freud, Wallon, and Piaget have adopted different but complementary points of view. I should like to expound briefly that of M. Piaget.

The stages are defined by two main criteria:

- (a) the process of formation or genesis;
- (b) the complete form or final equilibrium.

The equilibrium of a stage while marking the completion of one period marks at the same time the beginning of a new period of transformation.

Structures

M. Piaget has been able to demonstrate three types of structures:

- (1) sensori-motor 'group' structures;
- (2) concrete operation '*groupement*' structures;
- (3) combined 'group' and 'lattice' formal structures.

I will define each of these structures while describing them.

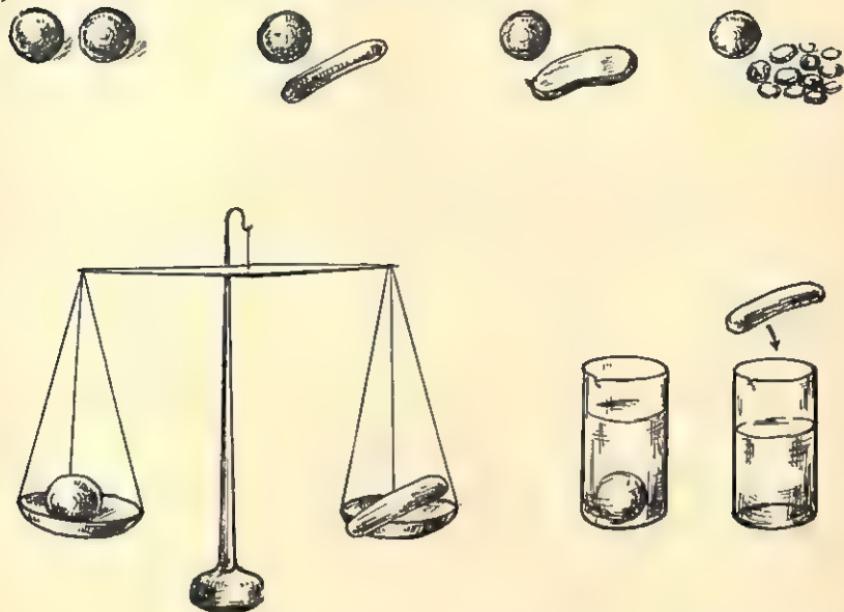
1. The structure consisting of a group of sensori-motor operations appears in the period of infancy and prepares for the stage of childhood. It is achieved at about one and a half years and is characterized by a system or group of reversible actions. The term 'reversible' is taken not in its medical but in its mathematical sense. Thus at one and a half years the baby is usually capable of making detours and of retracing his steps: in other words, of carrying out what Poincaré has called a 'group of displacements'. However, these actions are carried out by successive movements and not yet with the help of simultaneous representation.

2. The structure of concrete groupement begins in early childhood and reaches its equilibrium during later childhood. Actually it comes to full achievement between seven and eleven years. The concrete groupements which are carried out mentally permit the simultaneous, and not merely successive, evocation of a displacement or transformation and its inverse. Thus, for example, when a child transforms a ball of plasticine into a sausage or a cake (Fig. 7*) he can from seven years onwards mentally cancel this transformation and thus arrive at the conservation of matter. At about ten years he shows himself capable of carrying out the same reversible reasoning in connexion with conservation of weight and at about eleven years with conservation of volume. In each of these reasonings an actual transformation is cancelled by an inverse mental operation, thus leading to conservation. However, the child, unlike the adolescent, can only carry out one operative groupement after the other, not two simultaneously.

3. The structure of combined groups and lattices of formal thought marks the peak of adolescence. This structure develops between eleven and fourteen years and reaches its equilibrium at about fifteen years. The groups of formal operations integrate the partial groupements in a structured whole. The adolescent carries out a group of formal operations of the lattice type when he makes combinatorial analyses. At about fifteen years adolescents can make up a mixture of a number of chemical solutions not merely by chance but through combinations, associating each of the elements with all the others of the system. This reveals a new structure. Similarly, the adolescent can carry out a number of formal operations with a reciprocal

* I wish to thank Mr. Vinh Bang for the accompanying illustrations.

FIG. 7
CONSERVATION OF MATTER



and a negative corresponding to each, thus showing a group structure. The concept of proportion which the adolescent applies in the field of geometry and physics depends also on the group. The structure of formal operations thus shows an unlimited degree of reversibility and mobility.

Genesis

Having defined the structures we can now describe the stages of development as processes of formation leading to structures of equilibrium. I will limit myself to giving a few brief indications and examples.

The first stage stretches from zero to one and a half years approximately. This first period of life can be characterized by the genesis of the sensori-motor stage of intelligence and is achieved with the formation of the sensori-motor group. This consists of a combination of reversible actions such as displacements in space. Six sub-stages mark the gradual progress of development during this first period of life, with a gradual extension and increasing mobility of the 'schemata' of behaviour.*

* Piaget calls schema a piece of behaviour which can be repeated and co-ordinated with others.

- (1) 0-1 month: Reflex exercises.
- (2) 1-4½ months: Primary circular reactions (formation of motor habits and perceptions).
- (3) 4½-9 months: Secondary circular reactions (formation of intentional acts and prehension).
- (4) 9-11/12 months: Co-ordination of schemata (ends and means) and constancy of the object.
- (5) 11/12-18 months: Invention of new means (sensori-motor intelligence).
- (6) 18 months: Internalization of the sensori-motor schemata and achievement of the group of displacements (detours).

The second stage is characterized by a period of formation and a period of equilibration. The period of formation is from one and a half to seven years and the period of equilibration from seven to eleven years.*

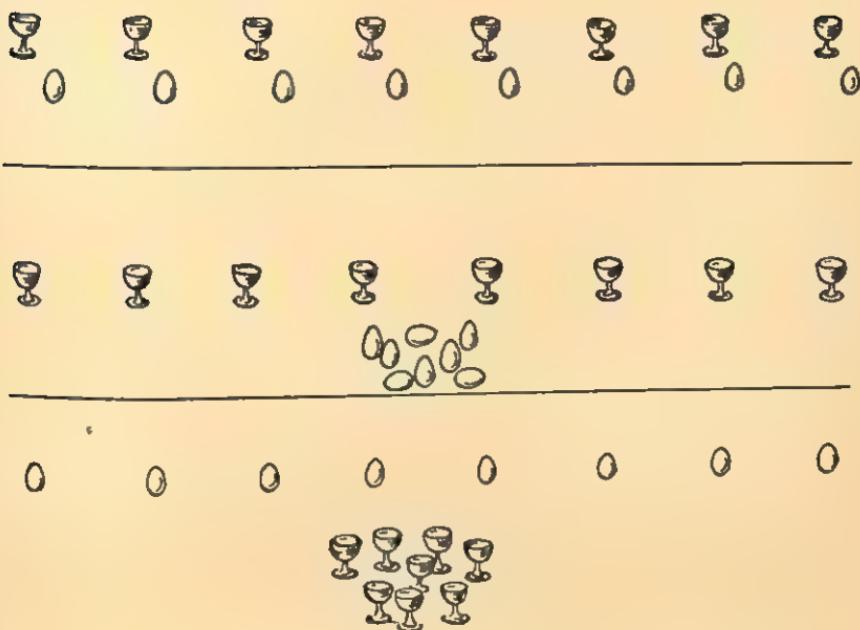
The formation period is characterized by the genesis of representative intelligence. Within this long period of formation can be distinguished two phases without clear demarcation.

The first is determined by the formation of symbolic thought leading to representation. Actually the change from sensori-motor action in the infant to mental representation in the child is due to the symbolic function which differentiates the significant from the significate. Everyone knows of the child's first attempt to represent events by symbolic play, drawing, and language.

The second phase is determined by the formation of concrete operations. Mental actions (internalized actions accompanied by representation) are irreversible before being grouped in reversible systems. Up to the age of six years the whole intellectual behaviour of the child is still determined by the irreversibility of mental actions.

* Since every equilibration phase is at the same time (in respect of the later phase) a preparatory phase, the phase seven to eleven years can be considered equally well as the phase of equilibration of the structures prepared between two and seven years and as the preparatory phase for the structures which are completed between eleven and fifteen years. Therefore we should speak of the stage from two to eleven years and another from seven to fifteen years, because structures follow each other with no definite break, since each new structure integrates the preceding ones and is prepared by them. The break at seven or eleven years is simply a question of convention or convenience. Here we use the following convention: stage I from zero to one and a half years; stage II from one and a half to eleven years with formation or genesis from one and a half to seven years and equilibration from seven to eleven years; stage III from eleven to fifteen years with final equilibrium reached at about fourteen to fifteen years. The levels of equilibration for concrete operations (particularly at seven years and eight to ten years) are numerous precisely because these operations are not yet formal, that is to say they are not yet entirely detached from their contents but constitute a progressive structuration of these various contents.

FIG. 8
NUMERICAL CORRESPONDENCE



I will quote as an example the behaviour of two boys, Vincent and Marco, who at regular six-monthly intervals were willing to undergo a psychological combined with an electro-encephalographic examination. We wished to complement our cross-sectional studies undertaken upon a large number of subjects by some longitudinal studies. The psychological behaviour of Marco at the age of five years six months and of Vincent at the age of six years is in fact marked by irreversibility in their reasoning. Here is a sample of the experiments which the two boys were submitted to.

For the first test (PIAGET and SZEMINSKA, 1941) a certain number of egg-cups and eggs are used (Fig. 8). The two boys had not the slightest difficulty in choosing from a basket as many eggs as there were egg-cups on the table. By means of an operation of a one-to-one and reciprocal correspondence they were able to place an egg each time opposite an egg-cup and so on. However, when the experimenter destroyed this perceptual correspondence by spacing out the eggs and putting the egg-cups close together or vice versa the children denied the existence of conservation of number and estimated the number of eggs as a function of the space occupied.

FIG. 9
CONSERVATION OF LIQUIDS



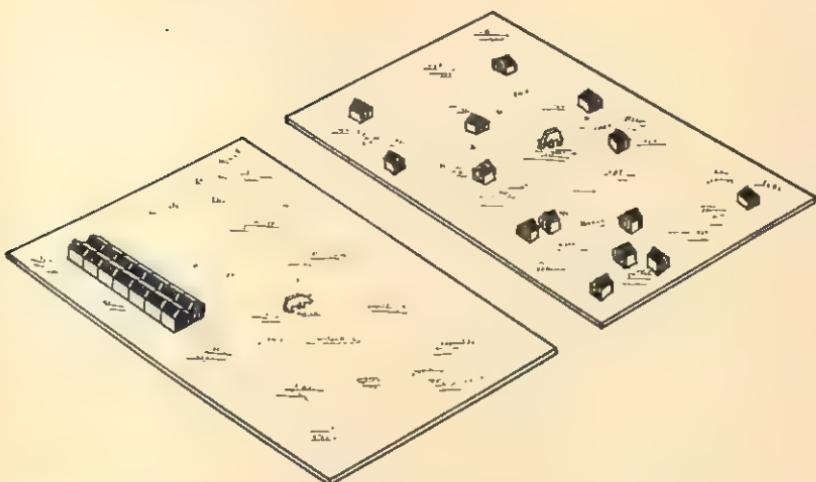
In a second test (PIAGET and SZEMINSKA, 1941) a liquid has to be poured from a tall, narrow glass into a low, wide one or else from a big glass into several little ones (Fig. 9) (for children we make believe it is a fruit juice we want to pour). Although they had themselves poured the liquid, the two boys believed that its quantity increased or decreased. The direct action of pouring could not be reversed mentally. In the same way the two boys were incapable of understanding the reciprocal compensation of dimensions (tall \times narrow = wide \times low).

FIG. 10
CONSERVATION OF LENGTH

For the third test (PIAGET, INHELDER and SZEMINSKA, 1948b) two rods of the same length are used. One of them is displaced parallel to the other (Fig. 10). Once again the two boys, centering their attention on the displacement of the rod, through lack of reversibility, thought it became first longer and then shorter than the other.

In the fourth test (PIAGET, INHELDER and SZEMINSKA, 1948b) two surfaces are used which represent fields on which two cows are

FIG. 11
CONSERVATION OF SURFACE



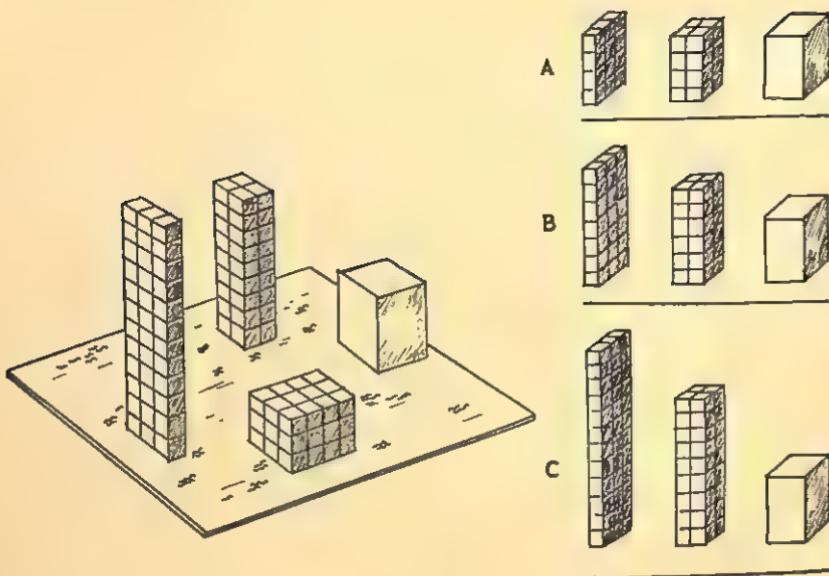
pasturing (Fig. 11). On each of the fields simultaneously we put a first house, then a second house and so on up to fourteen; only on one of the fields the houses are put touching each other whereas on the other the houses are spread out over the whole field. Now the problem is: Are the unoccupied areas of equal size. In the language of the child we ask: 'Have the two cows still got the same amount of grass to eat?' Here again it is because of lack of reversibility in their mental actions that the children are incapable of seeing the equality of the remaining surfaces.

For the fifth test (PIAGET, INHELDER and SZEMINSKA, 1948) a lake is represented with islands of different sizes on which the child has to build houses of the same volume 'with the same space inside' (Fig. 12). For the young children there is no question yet of making three-dimensional measurements. What interests us is to know whether or not they can think of compensating unequal dimensions. The two boys invariably constructed all the houses of the same height whatever the area of the base.

An absence of reversibility goes along with a certain rigidity in the systems of reference. This is why during the sixth test (PIAGET and INHELDER, 1948) Vincent and Marco were not yet able to imagine the water level as being horizontal in inclined flasks (Fig. 13).

In respect of a seventh test (PIAGET and INHELDER, 1941), and an eighth (INHELDER, unpublished), the two children had difficulty, which is characteristic for their age, in arranging objects in series

FIG. 12
CONSERVATION OF VOLUME



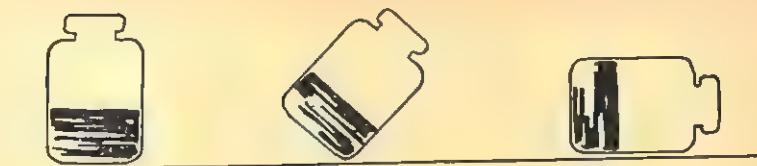
according to their size, or in classifying them according to two or three criteria at once.

In short, in these few tests, and in others, the two children were capable of carrying out mental actions but not yet mental operations, operations being by definition reversible mental actions.

After a slow continuous evolution the change from irreversibility to reversibility often occurs abruptly for a particular problem. Concrete operations as a whole, however, only very gradually impinge upon reality; the age of seven years marks only the beginning of reversibility.

The change from irreversibility to the first forms of reversibility occurred quite suddenly in Marco. After an interval of six months, that is to say between six years and six-and-a-half years, Marco's behaviour when confronted with the same experiments was completely altered. By means of a system of reversible operations he was able at six and a half years to understand certain invariances which he had denied at the age of six years. In the same way he was able to carry out operations of arrangement in series and of classification. The same change was more gradual but not less striking in Vincent's case.

FIG. 13
SYSTEM OF REFERENCE



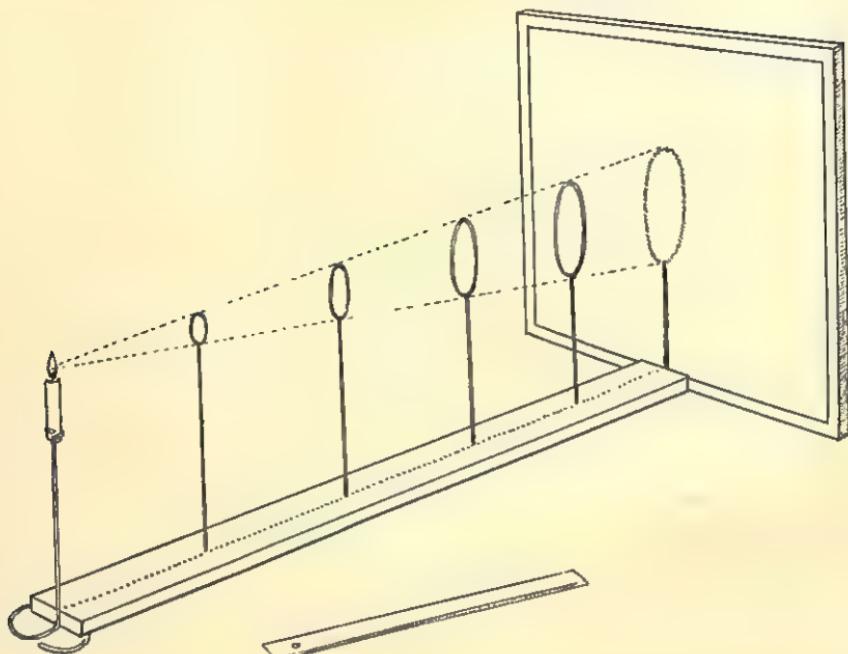
The balanced structure consisting of a groupement of concrete operations not only marks the conclusion of early childhood but serves as a basis for further development. Here can be distinguished different substages. At the age of seven years on the average the child is able to carry out logico-arithmetical operations (classifications, arrangements in series, and one-to-one correspondences) but it is a year later that the time-space operations are achieved (Euclidean co-ordinates, projective concepts and simultaneity). Thus up to about eleven years there develops gradually a system of concrete operations which will later serve as a basis for formal operations.

The third stage is characterized by the formation of formal operations which reach their state of equilibrium at about fourteen to fifteen years. At eleven years the pre-adolescent is already capable of deducing by means of hypotheses and not simply from concrete facts. His reasoning frees itself from the concrete. But only at about fourteen to fifteen years does this new form of intelligence attain a balanced structure governed by laws of groups and lattices.

In fact two boys, Philippe at the age of fourteen, and Udrea at the age of fifteen and a half, showed themselves capable of carrying out combinatorial and proportional operations, whereas a year earlier confronted with the same experiment they only proceeded by trial and error without reaching an exact solution to the problems. In the experiment mentioned earlier, developed with the help of Dr. G. Noelting (INHEIDER, unpublished) on colouring obtained by mixture of different chemical solutions, the two boys proceeded through a systematic combination of the elements presented. They combined the five colourless solutions in different orders: $1 + 2$, $1 + 3$, $1 + 4$, $1 + 5$; then $1 + 2 + 3$ and so on, with two, three, four, and five elements until they were able not only to obtain the colouring asked for but to discover the part played by a neutral and a reversing element.

In the projection experiment developed with the help of Mr. Vinh Bang (INHEIDER, unpublished) (Fig. 14) the two boys managed, at fourteen and fifteen years but not earlier, to produce a single shadow

FIG. 14
LIGHT PROJECTION



on the screen by means of a series of rings of different sizes placed at different distances, discovering, without previous teaching at school, that the size of the shadow is proportional to the diameters of the rings and inversely proportional to the distances from the source of light.

The formal type of reasoning of adolescents is thus disclosed, not only through verbal expression, but also by the way they organize an experiment and furnish a proof. The age of fourteen to fifteen years seems characteristic of this last form of equilibrium which brings about the completion of formal operations.

Criteria of Stages

In conclusion I would like to specify the criteria of the stages.

1. *The stages of development are defined by structured wholes and not by any isolated pieces of behaviour.*

The concrete groupement structure allows not only the solution of particular concrete problems but all the elementary types of classification, arrangement in series, and conservation of number. The appearance of a structured whole allows us to generalize from one

particular piece of behaviour to others of the same type. Unlike the tests modelled on the Binet-Simon tests which do not allow of any generalization since they proceed by summation of successes and failures, the appearance of an operational groupement allows us to identify a mental structure.

But there is more than that: structured wholes go beyond the operations actually carried out and are the base for a whole system of possible operations. We have seen that when confronted with the problem of combining chemical solutions the adolescent at a fifteen-year level proceeds to use a combinatorial method without any previous teaching. Thus not only does he recall the operations already carried out but he can construct a system of possible operations.

2. *The passage from an inferior stage to a superior stage is equivalent to an integration*: the inferior becomes part of a superior. It is easy to show that concrete operations serve as a base for the formal operations of which they are part. The combinatorial method, for example, is based on changes of order which are possible during childhood and later develop into combinatorial operations. Proportions themselves are operations applied to operations, or operations to the power two.

3. *The order of succession of stages is constant* but the age at which the structures appear is relative to the environment, which can either provoke or impede their appearance. The genetic development seems to follow a general law of the same type as the laws of organic growth. However, may I emphasize this: the age of realization cannot be fixed absolutely; it is always relative to the environment. The influence of the environment can act in many ways—at one time through the content to be constructed, at another by the possibilities of learning, or again by the social interchange itself.

The content to be structured: a group of objects may be more or less easy to classify according to their particular perceptual qualities.

Learning: it has been found that certain spatial representations are made easier by sensori-motor explorations.

Social interchange: certain comparative studies have shown that in an environment of free interchange and discussion magical representations decline rapidly in favour of rational representations, whereas they persist much longer in an authoritative environment.

These observations as a whole show the age margin which must be allowed for in our stages. Even if the intellectual development follows a constant order its manifestations are subject to fluctuation.

In summary we could say that the criteria of stages as shown by M. Piaget are based on structured wholes which follow one another in a constant order according to a law of integration.

The genetic conception of M. Piaget opens a number of new perspectives:

(1) The operational pattern of psychological structures may perhaps facilitate correlation with the neurological (cybernetic) patterns.

(2) Since the development of cognitive functions cannot be dissociated from that of affective functions, it will perhaps be possible to demonstrate their parallelism. M. Piaget has already shown the relation between the intellectual operation and social co-operation, as well as the interdependence of the pre-logic of childhood and moral realism.

(3) The establishment of a scale of development based on balanced structures will enable us to identify the level of operations in a child and not only individual successes and failures.

(4) The study of structured wholes is, however, insufficient if it is not complemented by research in differential psychology (sex, race, social environment).

FREMONT-SMITH :

Thank you very much. I watched Professor Piaget's face very carefully and he seemed calm and peaceful and at times even delighted. Mlle Inhelder's presentation is now open to discussion.

GREY WALTER :

I have been, in England and in my particular milieu, one of the most enthusiastic proponents of M. Piaget. I have tried to convey the ideas which have been developed here to my colleagues in physiology, with varying success, and I should like to put to you directly a question which is always put to me when I am trying to convince my colleagues that this type of behavioural analysis has validity in connexion with physiological problems. It is this: could you tell us very roughly how many children you studied, from what groups they were drawn, and whether you have subjected your quantitative results to any of the standard statistical analyses?

INHELDER :

I am not able to quote from memory the exact number of subjects examined in each of our studies. For the studies dealing with experimental reasoning in children and adolescents we examined individually more than 1,500 subjects from five to sixteen years. In some studies the examination of 100 children was enough to give us interesting indications, whereas for others 200 to 500 were necessary.

Moreover we are now taking up again with our students those studies which gave us the most significant results for the diagnosis

of mental development. We are working on a large scale and trying to study for particular age-groups the relations between our various results.

GREY WALTER:

What I would like to have is, for example, a set of distribution curves showing the range of the variation of these various behaviour standards with age, comparable with the curves for reading ability or arithmetical ability or the other factors which more conventional psychologists are accustomed to plot.

INHELDER:

One of our students is now preparing a thesis for a doctorate on this subject and he is studying the distribution of the results as a function of age.

TANNER:

All this work reminds me very much of the sequence of ossification occurring in children. Obviously, this is a large jump in analogy. Nevertheless, something similar does happen. The sequences of ossification are held to even if the child's development is slowed up. If I understand Mlle Inhelder and Professor Piaget correctly, one of the most cogent arguments for the existence of their developmental stages is that the sequence of them remains the same even if as a whole they are retarded or advanced. This is exactly the same with ossification centres, and this seems to me a powerful argument in favour of the existence of the mental stages, and of their neurological bases.

I want also to ask a question. Some children are advanced physically during the period of growth, and we have several ways of measuring the degree of advancement, of which two, I think, are of chief interest in this context. One is the state of the ossification centres—the 'bone age', and the other is the state of development of the teeth. At the present time my colleagues and I are doing a study in which we hope to relate these two things—to discover whether a child that is dentally advanced is also skeletally advanced. In general I am fairly sure that the two measures are not related to the same thing, though there may be a small correlation between them. The head grows rather differently from the rest of the skeleton, and the teeth are part of the head. Now does advancement or retardation in the sequence of mental tests relate to advancement or retardation in the teeth, or in the skeleton, or in neither?

INHELDER :

In the present state of our research I am unable to reply to that.

ZAZZO :

It seems to me that the main interest of the Geneva work lies in the establishment of the sequence and the explanation of the passage from one stage to another. This is a contribution of considerable importance to psychology. However, one problem arises, that of the curve of growth, and of the significance of the more or less rapid passages from one stage to another. Mlle Inhelder moreover stated the problem very clearly in her work on mental deficiency and she has already formulated several hypotheses. The problem I should like to underline is one of method. You said that the age at which the structures are realized is very variable and you give as the main reason the influence of environment. This is obviously one of the causes. I wonder, however, if there are not others. It has been noted for a long time that tests of mosaic or multiple-variety type enable us to determine the mental level with greater precision. One wonders whether Piaget tests should not be used in conjunction with other tests dealing only with an isolated aspect of behaviour. In all these tests one notes a surprising unrepeatability of results which arises, no doubt, from the educational conditions, diversity of environment and perhaps also from the conditions of the experiment. There are extraordinary variations related to the test situation, the experimental situation, and, in a general way, affective conditions. There is no doubt that this type of test is the most subject to affective fluctuations linked with experience. I think, therefore, that the difficulty of establishing a precise age for your stages comes not only from the fact that there is a wide dispersion but also in certain cases that an isolated activity does not enable us to obtain the same results as when a mosaic-type test is used. I should like to know what Mlle Inhelder and Professor Piaget think on this last point.

INHELDER :

I agree with M. Zazzo that there is a certain dispersion and even a certain lability in intellectual behaviour. Nevertheless, I am always struck by the fact that among developing children the dispersion within a stage is relatively small compared with the wide differences between behaviour in one stage or another, between one mental structure and another.

I am not so pessimistic as you as regards the unreliability of the results due to fluctuations in affectivity. It is above all important to

encourage each child to do his very best by creating an atmosphere favourable to the examination.

ZAZZO:

It seems that with these two types of test, the mosaic test and the Piaget test, we have this alternative: with the first type, we may determine a very precise age, within three or four months, but without knowing anything that lies behind it from the point of view of intellectual mechanism; with the second type of test, we may understand the intellectual mechanisms very well but not be able to fix an intellectual level. In the present state of affairs it does not seem possible by means of your tests to establish any kind of prognosis of development.

INHELDER:

I am entirely in agreement with you. In the present state of our research we are not able to say: 'such and such a child is exactly at the level of six years nine months'. However, I wonder if the compensations operating in mosaic-type tests are any more than compensations of a statistical order and whether they reveal the essential characteristics of an age level? That is another question.

PIAGET:

I should like to make a remark to the two previous speakers. The object of these studies, initially, was not to establish a scale of development and to obtain precise determinations of age as regards stages. It was a question of trying to understand the intellectual mechanism used in the solution of problems and of determining the mechanism of reasoning. For that we used a method which is not standardized, a clinical method, a method of free conversation with the child. We encouraged each child, as far as possible, in a way which was not comparable to that used with the preceding child. That is why, personally, I am always very suspicious of statistics on our results. Not that I dislike statistics; I worked on biometrics enthusiastically when I was a zoologist, but to make statistical tables on children when each was questioned differently appears to me very much open to criticism as regards the results of the dispersion. It would be very easy to make all this into a test-scale, but it would not have the value of a standardized piece of work like that which Mlle Inhelder's co-workers are undertaking now, for example. In reply to M. Zazzo, I think that by taking the operatorial mechanism for a particular level one attains something more general than the

mechanism of compensations in a mosaic test. I think that Mlle Inhelder has somewhat exaggerated the differences of age, the variations with environment, in order to be the 'Devil's advocate'. In actual fact, I have personally been quite surprised by the first results of Mlle Inhelder's students who are now carrying out a standardization in the form of tests. The regularity is greater than I should have thought judging by my clinical conversations with the children. I do not despair of obtaining a scale which will perhaps not be exact within three months but which, from the very fact that it will give a structured whole, will reveal more than would a system of compensations such as the mosaic-tests.

RÉMOND :

I should like to know if you have attempted to check the validity of your deductions by the 'educational artefact'. I will explain what I mean. A child from a known environment who has received the instruction normal to this environment is submitted to a course of training which might facilitate for him the acquisition of these stages. Have you tried to put yourself in conditions which are different from those of the children who are usually around you? In this connexion perhaps Dr. Mead could tell us if among children of a so-called primitive population, that is at least not having had a western education, one would find the same stages and the same ages defined by these stages. Taking the case of Negroes, for example, should we not find that the acquisition of these stages in American Negroes receiving the same education as the white people was normal, whereas on the contrary there would be a marked retardation among certain African peoples who have a very different educational system?

INHELDER :

I have not been able to experiment by varying systematically the educational factor or going so far as to carry out the experiment in a different cultural environment. My personal experience, apart from work on normal children, is confined solely to children whose schooling has been irregular, children who were refugees in Switzerland during the war. I noted that certain of these children at first contact gave a response which seemed to be at a level inferior to that which one could expect from them. However, after a quarter of an hour of experimenting and conversation these same children reached a higher level. In a certain sense they had caught up on their pedagogic retardation whereas the true mentally defective are never able to do this. Therefore it does not seem to me impossible

that in our cultural environment the method of clinical interrogation by means of concrete experiments should enable us to disclose the potential level of an individual. But I base this on only a few observations and cannot draw any generalization from it.

PIAGET:

I would like to give a second reply to Dr. Rémond. Let us take the last of the stages mentioned, the attainment of the formal level. There we have a whole structure which is characterized at first by the appearance of the logic of propositions, and if we study the lattices intervening in these propositional operations we find a correlation with proportions and combinatorial operations. From the point of view of the educational artefact it is surprising to note that proportions are taught, but combinatorial operations, at least in Geneva, are never taught in school. However, children invent these combinations by themselves. They find out not the formula, of course, but a complete method giving all the combinations for a certain number of variables. Here there are two operations which derive from the same structured whole, one of which is taught in school and the other not. They are, however, contemporaneous. It is striking that the school should have to wait till the age of fifteen before teaching proportions. I am certain that if teachers had been able to appreciate the concept of proportion in a more concrete manner they would have taught it already at eight years. If teachers have delayed this teaching until twelve years they have done so with good reason.

MEAD:

Before I can answer Dr. Rémond's question, I should like to ask Mlle Inhelder if I understood two or three of the points. Do I understand that you were working here only with sequences? Surely you have a lower limit below which you do not expect a given stage to appear? I would like to know how much possible spread you think occurs in development; do you conceive of some children reaching the six-year-old expectation at two, or at three or at five? Do you think of others not reaching it until ten?

The other question is the relationship to what we usually call intelligence. Do you expect a child with an IQ of 70 to show these stages in a rudimentary form, or do you assume that it will not reach stage 2 or stage 3 at all?

INHELDER:

As to the first question, I can only reply in a superficial way, letting you see later, perhaps, the necessary documents. In one of the

experiments on conservation solved by 75 per cent of the children at the age of six and a half, I did not find a single success until exactly five years and no failure after seven and a half. I was dealing, of course, with 'normal' children examined in day nurseries and in infant and primary schools of the town of Geneva. For other experiments the dispersion may be slightly greater or slightly less.

As to the second question, I have had the opportunity of examining retarded children and mentally defective children with IQ's of 70 and less. Among these children the true mental defectives, after a period of slow development, were able, at the age of physiological maturity, to pass the threshold of concrete operation structures (seven to ten years) but were never able to attain even the lower threshold of formal structures (eleven to twelve years). The imbeciles never reached the threshold of concrete operations. However, the IQ of 70 obtained by means of mosaic-type tests does not appear to me to be an absolute norm. I have found a few rare children who, despite an IQ of 70, were able to catch up on their retardation and to give in our tests results of a higher level. The IQ of 70 can in certain cases mask either a normal process unfolding slowly or a pathological process tending towards an early halt.

MEAD :

I am going to have to answer Dr. Rémond first on theoretical grounds rather than from observations. Stated as Mlle Inhelder stated it, this framework of stages seems to me to be at such a level of abstraction that it will probably be applicable to every people we know anything about. The sort of cultural variations that one would expect to find would be of this order. The Arapesh, a tribe in New Guinea, are people who only count to twenty-four, and after that say 'a lot'—they say one, two, two and one, one dog, one dog and one, one dog and two, one dog and two and one, two dogs—after six dogs they get bored. Now among such a people only the very brilliant are likely to use the same thinking as Piaget stage three, and it would take a considerable amount of extra education, and we don't know how much, to elicit from the average the same amount of facility in stage three that one would find here in this country. On the other hand, in another people not a hundred miles away in New Guinea and of the same general racial type who can count to thousands in their heads, can do the most complicated mathematical arrangements, and are exceedingly interested in adjustment to reality, I would expect many more individuals of average intelligence able to function as in stage three. Now, when I tried to apply Professor Piaget's formulation of twenty-five years ago, it did not work

because the formulation was far more concretely expressed than this formulation now. And when I tried to take magical thinking, the kind of animistic thought imputed to young children, and test it out among my second New Guinea tribe, then I found no correspondence. They were a tribe that had emphasized relationship to reality and factual reporting so intensively that the other type of thinking did not appear in children.

I think we need to distinguish between the cultural evocation of the lines of thought called primary process and secondary process thinking, and the way in which the sequence of development of these types of thinking is correlated with growth. I expect to find such an investigation cross-culturally useful, though at times it may be very difficult for a European to recognize the forms of thinking found in these different cultures.

The thing I don't quite understand, though, is the relationship of this formulation to the formulation of the child's relationship to reality, and the attribution of magical and animistic thinking to young children. That is, how do the two aspects of culture integrate or criss-cross?

INHELDER :

I can only make a statement concerning the environment in which I have worked. In fact we observe an inter-penetration of the pre-logical structures on the one hand with the animistic and realistic forms of childhood beliefs on the other hand. The appearance of the first structures of logical thought coincides with that of rational causality in the child. In our environment it seems that there is synchronism between these two processes.

PIAGET :

I think that in order to make comparisons between very different social environments, as Dr. Mead has done, it is necessary to find a system of tests as far as possible independent of language. All the tests which I used formerly had the drawback of being essentially connected with language. This is what I would call my pre-operational period. But if you take our tests on space, here is a field where one can find all the operations which can be presented relatively independently of language by a system of drawings, by comparison between a given concrete situation and drawings among which the subject can choose the true and the false. One should find something in common between different civilizations or cultures in the spatial field. These spatial operations are not, however, separate operations: one can find a whole series of groupements and other operations

applied to space. As long as no systematic comparison has been made between different cultural environments with these spatial tests one has great difficulty in separating the part played by language, with all its cultural significance, and the part played by operations. It seems to me this still remains to be done.

GREY WALTER :

We had an interesting experience with the application of the Piaget methods in the clinic. It is an anecdote rather than a report of the experiment, but it is of interest in relation to whether or not certain stages of development can be concealed or evoked by treatment or circumstance. We had some adult psychiatric patients who displayed extreme immaturity, with retardation in their E.E.G.'s and other physiological peculiarities, and we attempted to relate this finding with the stage of thinking. We did a series of tests which we based entirely on Piaget principles, and found, in fact, that patients who were in a severely disturbed psychopathic state had 'retarded' E.E.G.'s, and were very primitive in their behaviour, were not in stage three as they should have been, being adults, but in stage two of their development.

FREMONT-SMITH :

Had they never been normal?

GREY WALTER :

Yes, they had been normal people. They were aged between twenty-five and thirty and they had been perfectly capable up to a certain point, but they had regressed apparently back to a state where they could not do tests equivalent to simultaneous equations; they couldn't make reciprocal reversible relationships even in very simple tests. Some of them were treated and some got better and we could observe, in fact, a correspondence between the electroencephalographic changes and re-maturation and reacquisition of the capacity for formal reciprocal thought for the solution of simultaneous equations. Now we have just applied exactly the same test to a population of delinquent children. The application of Piaget-type tests to a delinquent population aroused such an intense emotional response in the subjects that the people responsible for the children suggested we should abandon the tests forthwith.

RÉMOND :

What were these tests?

GREY WALTER:

They were very much the same as those Mlle Inhelder was describing. We put water in glasses, we dropped a stone in the water, and so on. A particular study was made of the question of causality—we said, Why do you think the stone sinks?—and they said, 'Good God, if you think I'm such a fool as that . . .'—but they didn't answer.

ZAZZO:

As regards the question put by Dr. Mead, I think it is extremely difficult to separate the parts played by education and by heredity in development because the comparisons that we can make must always be carried out, even in primitive societies, in a human environment where there is language. Moreover, it appears that cultural and genetic factors are not additive. Their relationships are certainly much more complex. When reading M. Piaget's works I always wondered to what extent evolution and culture derive by a kind of maieutic process from hidden psychological aptitudes. In this respect, I would recall certain current jokes or certain well-established facts. It is said that the child of eighteen months is at the chimpanzee age and that at seven or nine he is at the Aristotle age.

You know of certain cases of wild children brought up by animals, notably the famous case of the wolf-child studied by GESELL (1941). The report published on the story of little Kamala is very interesting. At the time she entered the orphanage Kamala, who seemed to be about six or seven years, showed no human behaviour. She was re-educated under fairly strict control and at fourteen years, when she died from a uraemic crisis, she had reached the stage of language. This is a fascinating problem. It is obvious that when she was discovered in the jungle Kamala had not reached Piaget's second stage and yet other examinations showed that neurologically she was normal.

LORENZ:

I am sorry but I must lodge a passionate resistance against Amala and Kamala. I'll take my oath, and I want to drop dead this minute, if these children have really been raised by animals, and if you try to get hold of Gesell—as I did—he doesn't want to talk about it. Mr. Singh and Mr. Zingg, I am sorry to say, are people to whom the German saying applies, 'Wer einmal lügt, dem glaubt man nicht, und wenn er auch die Wahrheit spricht'.

If somebody assures me that a child raised by wolves has green luminous eyes, then I don't believe a word he says any more. A friend

of mine has caught them out in another untruthfulness. In ZINGG's book (1941) he proudly refers to an English scientist who, according to him, also refused to believe at first, but later humbly apologized. My friend, Dr. W. H. Thorpe, F.R.S., sought out that man, whose name I have forgotten, to ask him why he apologized and what had made him change his opinion. That poor fellow went wild with passion; it turned out that he had never apologized at all and did not believe a word of the whole story.

Now let me put before you a few of my arguments why I don't believe it. Supposing you have a wolf bitch who has lost her litter—if she had lost her litter there would be some chance of her caring for a child. She will grab that child and carry it to her lair, and then she will throw herself down and make herself ready to be sucked. That is all she does. She has no possibility of helping the child to find her teats. The child must be at an age where it doesn't yet grasp, because Amala and Kamala are reported to have eaten from the earth without using their hands (which a dog *does* by the way, it *does* use its hands in gnawing the bone, which Amala and Kamala surprisingly didn't, because neither Zingg nor Singh knew that dogs did). Then supposing that that child, by some incredible accident, happens by rolling about to find the teats, or that the she-wolf, by rolling about also, happens to bring her teats in contact with the child and raise that child, the she-wolf would suckle that child for two months, and then cease sucking it and feed it on regurgitated carrion. You must remember that she has to start at an age when the child still does not grasp or walk. And now I ask the paediatricians who are here what child taken by the she-wolf—let's be very generous and say at four months of age—suckled two months, and then fed by bitch-vomited carrion—what child would stand that?

MEAD :

Are all these details on the behaviour of wolves based on Indian wolves?

LORENZ :

Well, those are slightly shorter than the European ones, but otherwise they are the same.

MEAD :

And we have well authenticated details on their nursing and feeding?

LORENZ:

Oh, yes.

INTERVAL

LORENZ:

I have a rather long-winded question. It has to be long-winded in order to make my point clear. I am somewhat a heretic in regard to Gestalt psychology and it is my contention that Gestalt, in its most highly developed and most complicated forms, is nothing else but a phenomenon of constancy. When I see this paper in white colour, this perception is done by a very complicated apparatus working absolutely like a computer, subconsciously, and inaccessible to self-observation. This apparatus takes into consideration all other objects within the visual field, and assumes, as a working hypothesis which may not always be correct, that all these objects are reflecting light and colours, with no preference for any particular colour of the spectrum. On this assumption, the colour-constancy computer determines the relation between the colour of the impinging illumination and the colour reflected by the paper. This relation is, within certain limits, a constant, and this constant is characteristic of the object. This paper reflects, indiscriminately, all the colours of the illumination prevailing in this room; in other words, this paper is white. If the basic assumption of the colour-constancy computer is wrong, if the objects within the visual field do not reflect different wavelengths impartially, but some more than others, the conclusions necessarily are erroneous too. If the greater part of the visual field is filled by red objects, the computer assumes that it is the illumination which is red and concludes that a paper indiscriminately reflecting all colours of the spectrum, a white paper, must reflect green light in preference to red, in other words, that this paper is green. This miscalculation, logical in itself and only based on an erroneous premise, is the cause of what is known as the simultaneous contrast of colours.

Now consider what complicated operations the computer underlying our perception of form is able to make. They amount to the highest operations of stereometry. When I turn my pipe before my eyes its image assumes, on my retina, an infinite variety of different forms. Yet the form-constancy computer correctly interprets all these changes of form in the retinal image as movements of the object, and I perceive the form of this object as being perfectly constant. This amazing performance is even independent of the perception of depth! If I cast the shadow of the pipe on a screen, its rotation is still clearly perceptible, only the sense in which it turns has become ambiguous: Now let's go one step further and see how much more complicated

the function of our perception of form and movement can be. Suppose that my pipe, while I turn it around, should suddenly waggle its stem. I should notice it instantly—and wouldn't I be surprised! Suppose a duck, swimming before me on a pond, turns around; its image would be foreshortened, yet it would not appear shorter and fatter. Now suppose that that duck, while turning, fluffs out its feathers, so that it really would become relatively shorter and fatter. Even in this case, which seems confusing even in the telling, the two computers of form constancy and movement perception still succeed in keeping change of form and movement apart, though they have only the retinal image on which to base their computations. Our perception has not the slightest difficulty in interpreting the changes in the retinal image which are due to movements as movements, and those which are due to changes of form as changes of form. Now just hold this in mind for a moment and consider how complicated the apparatus underlying all these discriminations must be.

In phylogeny, it often happens that an apparatus, devised for quite definite and narrow ends, may prove useful in a quite different and much more generalized way of application. The human hand, evolved exclusively in the service of climbing, is the most commonplace example of such a change of function. Something very similar has happened to Gestalt perception. Maybe it is quite useful to remind English-speaking scientists, that the word Gestalt primarily is very nearly synonymous with form. And in phylogeny Gestalt perception has quite indubitably evolved out of the function of the form-constancy computer. Or, maybe, it is more correct to say it evolved out of all the constancy computers collaborating to build up our perceptual world. All these computers are objectivating in the most literal sense of the word. That is to say they always tend to determine qualities which are constant in and characteristic of the object. And that is all that they are reporting to the subject. The form-constancy computer does not report the accidental form which the retinal image happens to have at the moment, but quite directly the real, stereometric form of the object. This disregard for all concrete, but accidental, data of stimulation, the coming-down to constants essential for the object in question is very characteristic of all the constancy computers underlying our perception of things. That is what I mean by saying that they are objectivating. There is no doubt that all these objectivating functions have evolved phylogenetically in the service and under the necessity of recognizing individual objects or, to use a term coined by Karl Bühler, in the service of 'Ding-Konstanz'. But, as the human hand could be turned to new functions, the ability inherent in all constancy computers to extract essentials enables the organism to achieve something which comes

very near to the formation of abstract conceptions. It not only enables me to recognize my chow Susi independently of accidental circumstances by extracting her constant and essential individual characters. That process of 'extraction of essentials' can go one step further: without any change in the physiological—or, if you like, cybernetical—mechanism, my Gestalt perception is also able to extract essentials which are not only constant in and characteristic of that individual chow bitch, but of all dogs. It is able to disregard accidentals which are characteristic only of the individual, and to recognize one mutual, unconfusible Gestalt-quality of dogginess in this chow, as well as in the butcher's mastiff or my old aunt's peke. There cannot be any doubt that this direct perception of supra-individual Gestalt qualities is functionally closely akin to the formation of real abstract concepts. Very probably it always plays a considerable part in true abstraction and constitutes its indispensable basis. Cyberneticists tell me that an analogous possibility for an unexpected change of function has been found in calculating machines, too. A machine constructed exclusively for the purpose of calculating compound interest surprised its own constructor by being able to do differential calculus. The function of this 'abstraction-computer' is, after all, not more miraculous than that of the form-constancy computer enabling me to see the identical pipe in all those varying retinal images.

Wherever animals achieve something like a generic notion and whenever small children correctly apply generic names, they do so with the help of the 'abstracting' function of Gestalt perception. This is also the reason why little children often form generic conceptions with most unexpected contents. I knew a baby whose parents were quite desperate because that child 'couldn't tell the difference between a dog and a horse', because it said bow-wow to a dog and it said bow-wow to a horse and when confronted with a rabbit it said bow-wow too, but not when confronted with a goose or any other bird. It was clear at once that bow-wow simply meant 'mammal'. My friend Verlaine, in Liège, found that monkeys could, in a similar way, also achieve 'abstract' conceptions. An old and wise Javanese monkey could be trained to recognize birds, mammals, reptiles, and insects in coloured pictures and, at last, Verlaine taught him to make the difference between live creatures and dead things in general. This monkey's performance was so very extraordinary that some critics, among them my friend Otto Koehler, suspected that it did not react to the pictures that were shown to it at all, but to some signs unconsciously given by its master, just as the well-known 'talking' dogs and horses do. But I do not believe this at all, after having seen that monkey at work. With 'talking' dogs, it is always quite evident to

the knowledgeable observer that the animal does not pay any particular attention to the problem with which it is presented, for instance, to the characters or numbers that it is supposed to read, but evidently concentrates upon its master's face from which the signs guiding its performance really emanate. But that old monkey sat down in a frenzy of concentration with the picture in its hands, looking at it intently, and sometimes actually turning his back on the examiner. And after that he gave the correct solution in a statistically significant number of cases.

If you ask my personal opinion I am ready to assert that I quite believe in this monkey's ability to abstract, by the direct means of Gestalt perception, the 'conceptions' of mammals, bird, reptile, and so on. And one of my reasons for believing it is that my daughter, at the age of five, could do something much more difficult. In my own bird collection there were only two representatives of the order of Rails, or *Rallidae*. These two were the Coot and the Moorhen, both aquatic birds, adapted to swimming and therefore externally similar to ducks. On this statistically very misleading basis of induction, my little daughter's Gestalt computer enabled her to recognize any representative of the order of *Rallidae*. We examined her in the Vienna zoo, then containing a very rich collection. The Rail birds were kept together with other birds and distributed over a great number of different cages. Furthermore, you must know that the order in question contains a lot of species which are, in body proportions and general appearance, as different from each other as could be. There are long-legged forms looking much like herons, duck-like aquatic species, and some dry-land forms that look exactly like gallinaceous birds, for instance, like quails or partridges. Nevertheless, my daughter picked out the right birds without a single error and without any suggestive questioning on my part. On the contrary, I tried to mislead her, showing her cages in which there were no Rails at all, but some externally similar birds, asking her in a voice suggesting that there must be some, but she flatly asserted there weren't any! When I asked her how she recognized the Rails for what they were, she could only say, 'Well, they are just like a moorhen . . .'.

Now the question to which I am leading up, and which is directed particularly at Mlle Inhelder, is this: I believe that among your intelligence tests for children there are some which investigate the child's ability to abstract, to generalize. Do you ever, in such tests, find any correlation between a person's power of abstraction and his or her particularly ability to perceive complicated Gestalts? If what I suspect is correct, you ought to get in this respect a particularly big variability in tests of generalizing power, because it is well known

that the ability to see Gestalts as wholes is very different in different men and different types of men, and if there really is a strong causal connexion between Gestalt perception and generalization there should be a very large variation in the behaviour of children in the generalization tests.

INHELDER :

In this question we return to the form of reasoning and the content to be structured; the faculty of abstraction does not correspond to an operation which would appear at seven years whatever the object or the situation. Although a child of seven years may manage to classify red boxes and oppose them to the class of blue boxes, and may be able to classify big boxes and oppose them to the class of little boxes, I cannot tell without previous experience whether he would be able as easily to form a class of primroses and to oppose it to the class of non-primrose flowers. In the same way the hierarchical pigeon-holing of animal classes (in our experiment it is a matter of ducks, birds, and animals) can be greatly facilitated when we oppose a non-animal class (chairs, books, etc.) to the animal class.

The moment when the classification operations appear depends not only on the operational capacity as such but also, though in a lesser degree, on the content on which it operates. Obviously this is only a partial reply to your question.

PIAGET :

Dr. Lorenz spoke of a case where the common Gestalt encourages abstraction, whereas in most of our tests we try to put the perceptive configuration in conflict with the operation. That is why we have difficulty in replying to you.

LORENZ :

Yes, in order to reply to my question it would be necessary to carry out tests where the Gestalt is absolutely useless.

PIAGET :

Without wishing to detract from your daughter I would say that a common Gestalt encourages abstraction and then we are not certain of being in the presence of what we call an operation.

GREY WALTER :

It can be shown that a machine or animal that can extract invariants in the form of Gestalten, or patterns, is automatically capable of learning. A machine or animal that has been designed, or has

evolved, so that it can learn can automatically at the same time extract Gestalten. This is a first principle of cybernetic design which has been developed only quite recently as a theorem.

LORENZ:

I am very excited about this cybernetic fact. Learning facts like that makes it worth while attending conferences! I may somewhat lessen my excitement by stating that WOLFGANG KÖHLER (1940) knew one side of this and said that no animal can acquire knowledge of a stimulus situation as a signal releasing a conditioned reaction if it is not within the scope of its Gestalt perception. This works only one way, though, and it seems tremendously important if it can be shown that, conversely, a machine that can perceive Gestalt is automatically able to learn.

MEAD:

This is a very good illustration, I think, of an intervention of cultural factors. The two illustrations of Dr. Lorenz are perfect examples. In the one case you have the small child who recognizes a mammal, and his parents are distressed, so they say 'No, that's a dog, bow-wow here', and they break the abstraction down to a concrete level. On the other hand, Dr. Lorenz's child, having grown up in a home where abstraction was valued, does the same thing and it is valued. We have a good many instances of this happening. The fourth word my child said was an abstraction. She said the word 'baby' for her reflection, a doll, a photograph, and a carving. Because we were interested in linguistics and were interested in concept formation we saw what she had done and we said it back, so it had some chance. So in your concrete cultures, in languages where there is a word for 'snow in the air' and another for 'snow on the ground' and another for 'snow that's crisp' and another for 'snow that's soft', undoubtedly the child at two sees snow and says 'That's snow' and is told 'No, you don't see snow, there's no such thing, it's this thing and this thing and this thing'. Then, when we come to the adult in various societies, we see one people very able to abstract and another people very handicapped in that respect.

LORENZ:

I think culture influences Gestalt perceptions even more because you have to look at something very often until the Gestalt jumps out at you. For example, the regularities contained in Eastern music are imperceptible to our Gestalt perception which is attuned to the eight-tone octave and won't respond to the scale of the Asiatics, and

to us Asiatic music seems at first to be just chaotic. I doubt very strongly whether a grown-up person who is not particularly musically gifted is able to learn to feel the Gestalt and thus the beauty of Chinese music, and it would be an interesting experiment for Gestalt psychologists to see at what age you are still plastic enough to learn to do so.

MEAD :

Let me come back to the theoretical point which you raised. The people who appear to us to see least are the Australian Aborigines, who apparently relate themselves to their environment much more by touch and by smell than by seeing. Australia has an extraordinarily monotonous landscape. The drawings of the Aborigines are always diagrammatic; they are not pictorial representations. But take Aboriginal children now and show them perspective and they get drunk. They get into an absolute ecstasy when they first encounter perspective, and small ten-year-olds will light up and say 'I see, you've painted it the way it looks, not the way it really is'.

CAROTHERS :

It seems to me that in our Western European world the reality is essentially one of spatial and mechanical relations. The most striking thing one sees in Africans who have lived in the country and are not familiar with a European background is their utter incapability of dealing with spatial relations. I have tried to test Africans with blocks of wood, coloured on different sides: a cube-imitation test, a very simple thing for Europeans, even perhaps at about the age of eight; but the African adult, on average, is quite incapable of dealing with this, and I finally came to the conclusion that most of the people I examined did not realize that, when they turned a cube round, the various sides maintained a constant relationship to each other. This is for us a very simple thing, and these Africans were in many ways intelligent, capable of dealing with their own environment extremely well, but in this skill, which is acquired by us I suppose in playing with toys, they are feeble-minded by our standards. Reality for them is not a world of spatial relations, but a world of spirits, and our mechanical world is much less real to them.

MEAD :

I think it is important to distinguish between the word 'reality' as it is usually used in psycho-analysis or therapeutically, when you say you have to face reality—which includes your mother-in-law, the marriage systems, the secret police, or whatever happens to be

around in your country—and reality in the sense that Dr. Lorenz was using it, when he said that the function of this mechanism was to inform the animal of the actual state of the object. Unless we keep those two things absolutely clear, we are going to get into a great deal of trouble. To date, I have seen no evidence anywhere of any people with an inability to interpret whether an object is moving or not (that is to function as Dr. Lorenz was describing) unless superimposed there is a theory of spirits, which then gives a new interpretation. But this is quite different from the original capacity to perceive. My interpretation, on the basis of the testing I have done with different primitive people, would be that there is something in Dr. Carothers' test which evokes an alternative interpretation in the African. After all, when the African turns his boat around he knows all about the sides. When he is dealing with any object in his own environment, he knows just what he appears not to know in that test situation. We have to think of culture as mediating between the actual nature of the object, reported in Dr. Lorenz's sense, and the final interpretation as to whether it is a ghost or a devil, or whether it was there or wasn't there.

CAROTHERS:

I think it is true to say that hearing plays a much more vivid part in the African's life than vision. He is familiar with the sounds of the wind in the trees, the running of water, the noises of the animals, and the speech of his relations and his friends. He is not nearly so insistently introduced to visual aspects of the world as European children are—reading, writing, playing with toys and blocks of wood. It seems to me that all these auditory stimuli are of a very emotional type. They are always relevant to the person. No matter what sound one hears it is always something that is of immediate interest, and even the human word as spoken is far more emotional, and of far more intense personal interest, than the written word. That is likely to play a part in the African's approach to life and to emphasize his subjectivity and his tendency to egotism and his lack of detachment. The visual world is also far more continuous and always reminds one of the fact that each event depends on previous events, that things do not happen haphazardly; whereas in the world of sound things are discontinuous and cause and effect are far less obvious.

PIAGET:

From the discussion which has just taken place I understood that everyone agreed that according to the different social or cultural

environment variations in mental development occur as a function of education, emotional forces, etc. Nevertheless it seemed to me that Dr. Mead and Dr. Lorenz postulated a sort of constancy of Gestalt in the immediate sensorial universe, throughout the world. I personally have some doubt on the subject. I do not think that all the Gestalten are the same for both child and adult. I will give an example from the constancies referred to by Dr. Lorenz when he was explaining his ideas on Gestalt.

We carried out some research on the depth evaluation of real and apparent (projective) size. As regards Euclidian or real size we found that the constancy was lower in the child—as was demonstrated long ago by the psychologists of the Vienna school. The child undervalues depth. At about ten or eleven years he attains approximately the level of real constancy. The adult overestimates depth. There is an over-constancy in the adult and this over-constancy is the rule. Many psychologists had demonstrated this, but with a certain constraint, passing over this phenomenon like a cat on hot bricks because it cannot be reconciled with Gestalt.

Now if apparent size is studied, that is, the way an object at four yards appears compared with an object at one yard, for instance, it is found that the adult has considerable difficulty in evaluating this projective size. He makes an error of about 50 per cent, and even more. In the case of a child of about seven years one has great difficulty in getting him to understand the question, but once he has understood it his perceptive evaluation is better than the adult's. His estimate of the apparent size is in some cases 100 per cent. In adults we have only found one case where the evaluation is as good as the young child's, and he was a landscape painter. Apart from this exception adults have a very inferior power of evaluation compared with children.

There is a large number of phenomena of this kind, which shows that Gestalt, propounded as a simple, universal explanation, appears to be a myth from the point of view of genetic psychology. Of course, I readily admit that these phenomena depend not only on age but also on culture, and I think that even from the sensorial point of view wide differences are found.

MEAD :

Some of you probably know of the experiments of AMES (1952) and the room he has constructed so as to give an illusion of being rectangular when actually it is extremely distorted. You go in and then you are given a stick and told to touch first this part of it, then that part of it; and it takes a long time for the motor experience

to correct this visual illusion. In this little house there are two windows of the same size, so constructed that when you look at people looking through them one person looks almost twice the size of the other. If we took members of this group into this experimental situation, if there were two of you behind the windows and one of us looking, we would get the illusion completely and accept the fact that the whole situation had been set up for it. But with a child looking at its parents, or a wife looking at her husband, the distortion does not operate and they see them with the whole perception corrected, so that there is not only the deceptive constancy of the adult but the constancy that is given by the emotional situation. (We are not quite sure what husbands see in wives, because the experimenters have a theory which relates to dependence and they maintain their theory by not having husbands looking at wives.)

LORENZ:

If a husband sees his wife the right size then at the same moment he ought to see the house askew.

ODIER:

I might relate an experience with a boy. He saw his father and mother coming up an avenue and he said to me, 'Isn't it funny, why is daddy so small and mummy so big?' Actually the mother was an inch or two shorter than her husband. The child, who was passing through the Oedipus phase, had an emotional tendency to diminish the size, i.e. the value, of his father. I think this is an illusion, a kind of 'disgestaltism', frequently found in psychoanalysis.

I would like to make another remark in connexion with Mlle Inhelder's very interesting communication and M. Piaget's remarks. Many of you commonly speak of tests, but I think a distinction could be made between an actual test and what we might call an inquiry. The way Mlle Inhelder and her students go about getting children to undergo testing seems to me different from the cold and automatic way testers apply tests just as they are to all children without discrimination. As I understand it, Mlle Inhelder prepares the ground; with each child she tries to create some relationship with an affective component by explaining things to him and playing with him. In psychoanalysis we would say that Mlle Inhelder attempts to create a positive transference. Now I have ascertained that in certain cases a negative transference is established and this affects the result of the test.

I will quote you an actual case which borders on the anecdotal. I had occasion to supervise the work of a child psychoanalyst. She was

treating an anxious boy of seven or eight who had been tested with the communicating vessel test by one of the Institut Rousseau students. This test consists in showing a child two communicating vessels. One of the vessels is screened and the water-level is varied. The child is then asked various questions to reveal the extent of his acquisition of the concepts of dynamics, volume, and horizontality. The child gives all kinds of interesting replies of an animistic or magical type, saying that the water is growing, etc. Well, this child finished by telling the psychoanalyst that he had been careful not to say what he thought. He had imagined that the tester wanted to know whether he had wet his bed.

I have seen another case where a test included filling flasks with water and emptying them. The little boy was suspicious. He made a negative transference and replied all wrong because he thought the young lady—as he called her—wanted to know whether he had masturbated. Naturally in this case the result was falsified.

These examples clearly show that two different factors must be distinguished in these tests and that a so-called intellectual test must not have an affective base, and inversely an affective test must not be carried out on an intellectual plane.

CAROTHERS:

I want to ask Mlle Inhelder if she envisages these steps in mental development as definite steps with a pause in between, or does she envisage the mental development as continuous and these steps simply as milestones of measurement?

INHELDER:

The evolution appears to be continuous; however, in this continuous process there seem to be decisive periods: for example, at about seven years, which appear as accelerations. It all happens as if a slow preparation suddenly culminated in an achievement.

FOURTH DISCUSSION

Comparative Behaviouroulogy

LORENZ:

If you pull a chair away from a man sitting on it and balancing crockery, he will put out his hands and let the crockery drop. This is due to a reflex. I am convinced that the thing called a reflex exists, but I am also convinced that it is very far indeed from being the only basic function of the central nervous system, as many physiologists still believe. Someone once said that today's truth is tomorrow's error. Otto Koehler very wittily and very profoundly replied, 'No, today's truth is tomorrow's special case'.

Most accepted 'truths' about the reflex are such special cases. None of us knows what is really happening within the central nervous system. Permit me to talk in parables. Supposing there is an automatic machine on the railway station. You put in a coin, then there is a mysterious buzz and out pops a package of cigarettes. The machine is the central nervous system, the coin is the stimulus and the cigarettes are behaviour. The buzz represents everything recorded by electrophysiology, and is the only hint we get as to what may be happening inside.

Now let us, for the moment, forget all we think we know about the approved 'truths' concerning the reflex. Let us regard the central nervous system as such an automatic machine whose insides are quite unknown to us. Let us study its different types of performance, its different types of reaction to stimulation, and also its spontaneous activities, and let us classify them from the point of view of the stimulation impinging on the central nervous system and of the activities elicited by these stimuli. One of the most common types of central nervous performance is as follows. A continuous and rather amorphous stream of excitation comes in through afferent nerves and the output is a very well co-ordinated rhythmic sequence of motor impulses. Many instinctive movements, particularly those of locomotion, belong to this type. Or one single impulse is sent in and a series of rhythmic impulses constitute the response as, for instance,

in the case of a clonus. Among literally hundreds of such combinations between the impinging stimulation and the resulting efferent impulses, there is one case in which there is a direct correspondence between stimulation and efferent impulse, that is to say one stimulus elicits one efferent impulse, thus creating the extremely misleading impression that the central nervous system has not done anything but pass on a stimulus from one neuron to another; the prototype of what is called a reflex. This kind of central nervous response is neither common nor widely spread in the zoological system. On the contrary, the most typical and classical form of reflex, on which the whole conception was based, has evolved very late in phylogeny, as it only exists in animals with a pyramidal system.

Once we have recognized it as one special case amongst hundreds of different ones, there is not the faintest reason to believe that all central nervous performances are based on 'the reflex' as a primary functional element; yet with some physiologists, you will find it extremely difficult to overcome this opinion!

If we put the typical reflex—one stimulus, one efferent impulse—at one end of the diagram which classifies central nervous performances by their input and output, we find, at its other end, a number of functions which are independent of impinging afferent stimulation. This endogenous or automatic production of impulses may result in the output of a continuous stream of simply amorphous excitation, as has been shown by HOLST (1939, SCHOEN and HOLST 1950) for the sensory cells in the macula utriculi in the labyrinth of fishes, and termed 'autostaxis'. Or it may result in sending out very complicated and highly co-ordinated rhythmical series of motor impulses. Holst not only deafferented, but totally isolated the central nervous system of the earthworm, and found that it still persisted in sending out rhythmical co-ordinated impulses. These impulses proceed in waves from the rear part of the front end and are indubitably identical with those normally eliciting the creeping movement of the worm. When this was doubted by some critics, Holst proceeded to isolate only part of the nervous system and to leave part of it in connexion with several segments of the worm. Then the action currents conducted from the isolated part to a series of galvanometers went beautifully and convincingly in step with the contractions of the intact segments. This type of central nervous activity, producing, without any afferent stimulation or control, rhythmical and co-ordinated movements, was called 'autorhythmia' by Holst. Pure autorhythmia is, in the animal kingdom, about as rare as pure reflex activity, but it is indubitably a much more primitive phenomenon. It is very probable that all locomotion in protozoans is closely akin to autorhythmia, and Bethe has shown conclusively that the contractions of

the umbrella of some jellyfish are autorhythmic. But in some more highly differentiated types of jellyfish, the *Scyphomedusae*, the autorhythmia of the velum has been gradually superseded by a typical reflex mechanism. These animals developed static organs called marginal bodies which primarily served a righting response by stimulating a stronger contraction on the lower rim of the umbrella whenever the animal was tilted to one side. But gradually, in a manner which need not concern us here, the reflex mechanism of this righting response has replaced and superseded the autorhythmia of the velum. Thus the jellyfishes, meek animals though they are, may serve as a warning to the physiologist never to generalize rashly. The umbrellating movement of a primitive *Hydromedusa* not only looks exactly like that of a *Scyphomedusa*, but actually is the 'same' movement ontogenetically and phylogenetically. Yet the one is caused by the purest type of autorhythmia that we know and the second by the purest type of reflex that we know. I confess, however, that it gives me some satisfaction to remind you of the fact that autorhythmia is certainly the more primitive and the older of the two.

I deemed this excursion into neurophysiology necessary in order to explain why ethologists absolutely refuse to accept the current explanation that all instinctive activities are 'chains of reflexes'. No complex instinctive movement has, as yet, been really analysed down to its neurological components. Yet there is overwhelming evidence that true autorhythmia plays the most important part in the causation of the typical spontaneity of all instinctive activity. This assumption supplies us with a most convincing explanation of a very great number of facts concerning instinctive movement, while the same facts remain totally unintelligible on the basis of the chain reflex theory. And the fact that all instinctive activities are fundamentally spontaneous and therefore akin to autorhythmia is relevant for what I want to say to you about what we call the innate releasing mechanism (I.R.M.). As long as the elaborate and well co-ordinated sequence of movements constituting an instinctive activity was regarded as a chain of reflexes, its beginning did not obtrude itself as a problem: it was just the first one among other reflexes and the existence of a neural mechanism of a peculiar character did not become apparent. But as soon as it became evident that the elementary nervous functions underlying instinctive movements were much more akin to autorhythmia than to reflex activity, a new problem put itself very directly. If the instinctive activity is generating impulses continuously—as there is plenty of evidence that it does—and prevented from causing incessant movement only by the inhibiting function of higher centres, how is this inhibition removed at the right moment and in the biologically adequate situation in which the instinctive activity must be

discharged? We know that the ventral chord of the earthworm generates the impulses of creeping incessantly, yet the intact earthworm creeps only when it ought to.

There is no contradicting the assertion that the 'trigger-mechanism' removing the central inhibition which, most of the time, prevents instinctive activities from 'going off *in vacuo*' is nothing else than an 'unconditioned reflex' in the original sense of I. P. Pavlov. Yet this assertion does not solve the real problem, which actually lies in the selectivity of the releasing mechanism. This central problem only becomes apparent in the more complicated forms of the I.R.M., not in the simple cases that Pavlov's classical examples provide. When a dog secretes two different kinds of saliva on having meat extract put in his mouth in one case and sand in the other, the difference between the two reactions is easily explained by the fact that the meat extract stimulates chemical receptors and the sand tactile ones. But take the following case as a contrast. A young totally inexperienced jackdaw instantly reacts by doing 'social grooming', i.e. preening the other's feathers, if another assumes the 'friendly' or submissive attitude which consists in turning away the beak and fluffing the feathers at the back of the head. But if the other bird instead of fluffing the feathers of the head fluffs those of the back, and instead of turning the bill away turns it towards the approaching youngster, the latter will instantly 'recognize' this attitude for that of a threat, and react either by retreating or by a counter-threat, according to its relative strength and courage. To anybody not believing in vitalistic miracles, these two different ways of reacting to two stimulus situations which, after all, are only different in regard to the retinal images received of the submissive and the aggressive jackdaw, must be a source of deep wonder. We have to postulate an innate perceptual structure which acts as a sort of filter letting through only sharply defined combinations of sensory data. Thus the problem does not lie in the question of whether this performance is due to an unconditioned response or not; it lies, so to say, in the afferent side of the reflex arc.

If we do not know what an innate releasing mechanism (I.R.M.) really is, we know quite a lot already about its function. A lot of investigators, chiefly Tinbergen and his school, have concentrated on the I.R.M., so that at present it really is the best known element of innate behaviour. I cannot give you here even a short survey of everything we think we have brought to light about the function of the I.R.M.; I only want to emphasize a few points.

(A) An I.R.M. never responds selectively to a complicated Gestalt, but exclusively to extremely simple key stimuli. These may be relational properties, but always such simple ones that the relation in question can be stated in very few words. For example, the I.R.M.

which, in the young herring gull, elicits begging activity, responds to the following stimuli emanating from the parent bird's bill:

- (a) The narrowness of the lower mandible which, in feeding, is turned with its sharp under edge towards the baby bird.
- (b) The red colour of a round spot on the lower mandible of the parent bird.
- (c) The contrast of this spot against the paler coloration of the bill.
- (d) The downward tilt of the bill.
- (e) Its jerking movements while the parent bird is regurgitating food.

The general form and colour pattern of the parent's head, bill, eyes, etc., are quite irrelevant for the releasing power of the situation.

(B) If, as in most cases, an I.R.M. responds to more than one key-stimulus, the effect of stimulation is exactly in proportion to the sum of the efficacies of the several key stimuli impinging at the moment. This rule, termed the 'law of heterogeneous summation', constitutes a very striking difference between the learned response to Gestalt qualities and the innate reaction of key stimuli. Furthermore, I want to draw your attention to the fact that this very limitation of an I.R.M.'s functions, particularly the limitation of their complexity, is just what one would postulate from a purely mechanistic, or, if you like, cybernetic point of view. It is quite to be expected that a 'receiving set' reacting selectively to a few and very pregnant key stimuli is easier to construct than one which reacts to a diffuse complex quality of an elaborate Gestalt. Indeed, the very simplicity of the I.R.M. is a reason for analytical optimism! Though our ideas about what the I.R.M. really may be neurophysiologically are purely conjectural, I think that the construction of a cybernetic machine of similar functions and limitations would be very illuminating.

As our ideas about that peculiar stimulus-selecting apparatus which we call the I.R.M. are largely founded on our observations about what it cannot do, it will not seem surprising to you that the pathology of the I.R.M. is also one of the major sources of our knowledge about it. After all it is the approved method of neurophysiology to study the pathological defects of a function in order to gain insight into its normal causal structure. Another reason for enlarging a bit on the pathology of the I.R.M. on this occasion lies in the fact that it might have some bearing on some phenomena of delinquency in human social behaviour.

One basic fact which is extremely characteristic of the normal function of all I.R.M.s must be stated first of all: in all cases in which a pathological disintegration of an I.R.M. is found, the activity normally released by that I.R.M. does not, by any means, become

unreleasable, but, on the contrary, more easy to release. This fact very strongly enhances the I.R.M.'s character of a 'filter' of stimuli: the more that 'filter' is broken up, the greater becomes the range of stimuli which are able to pass through it and to release the reaction in question; in other words, the less becomes the selectivity of the response.

There are three different factors which regularly lead to a disintegration of the I.R.M. and to a corresponding loss of its selectivity:

(A) Any, even a very slight, disturbance in the general state of health of an animal.

(B) The domestication of a species.

(C) The hybridization of two species with slightly different I.R.M.s.

Let me first give you an example of (A). The red-backed shrike (*Lanius collurio* L.) has an instinctive movement to impale insects and other prey on thorns, in order to store food. In young birds of that species, reared by me in isolation, I found that the innate movements of impaling were not innately directed towards a thorn. They tried to impale a mealworm or a small piece of meat indiscriminately everywhere; on their perches, on the bars of their cage, etc., without giving the slightest attention to the very adequate artificial thorns with which I had supplied them. Only when, by pure trial and error, they happened to execute the innate movement pattern of impaling on one of these thorns, the full success of the instinctive movement evidently acted as a reinforcement to direct the impaling activity to the adequate object, and they learned with extreme rapidity. Naturally, my conclusion was that the red-backed shrike had no I.R.M. directing its impaling activity to the thorn, but that it had to learn its proper use by individual experience. This, however, proved to be quite erroneous. Kramer, of Wilhelmshaven, reared young shrikes in order to study their migration activities. Just because an easy opportunity offered itself, he repeated my experiments on the impaling activity—with exactly the same results. But one year later he improved his rearing technique, feeding the young birds on a large proportion of live silk-worms. And when he repeated our experiments with these birds which were in just slightly better condition than all those previously used, he found that they had an innate reaction to the thorn! At the very first experiment they took the mealworm in their bill, looked about for an adequate thorn, recognized it instantly when they saw one, went straight for it and, without the least evidence of trial and error, impaled their worm on the thorn, just as if they had done so hundreds of times.

Now an example of (B). In the wild ancestor of our domestic chicken, the jungle fowl or bankiva, the mother hen refuses to

brood any chick which has not got the typical wild colour pattern of down on its head and its back. Some bankiva hens instantly kill any black chick. In domestic hens one finds all possible gradations between this extreme selectivity of the I.R.M. releasing the brooding activities, and an entire lack of selectivity. Most barnyard hens are insusceptible to all the possible colour patterns in chicks, but some will refuse to brood ducklings or goslings; highbred races, like cochins or brahmas, will brood practically anything alive and approximately of the right size—young ferrets, for instance.

Now to come to (C), the loss of selectivity of the I.R.M. in hybrids: some hybrids of bahama pintail and chestnut-breasted teal that had been reared normally, in company with their brothers and sisters, did not react sexually either to each other or to any birds of the two parent species, but all of them, both males and females, courted a huge white-spotted domestic duck, about four times their own size. Similarly, Heinroth found that some goose hybrids persisted in courting swans. Though these are just a few isolated observations, they tend to show that loss of selectivity of the I.R.M. stimuli may result in the choice of the *strongest* stimuli available.

Finally I want to mention one peculiarity of the I.R.M. which, though not pathological in itself, may lead to phenomena closely akin to the pathological. In most I.R.M.s, the stimulus-receiving set is tuned to the quality rather than to the quantity and intensity of the natural key stimuli. Therefore most key stimuli can be exaggerated. In the jewel fish, *Hemischromis bimaculatus*, the dark ruby-red colouring of the male's throat is one of the key stimuli releasing fighting activities in a rival. By illuminating our fighting arena with ruby light, thus intensifying their colour, we can make jewel fish males 'see red' and fight each other more intensely than under normal conditions. If more than one of the key stimuli emanating from a certain object can be thus exaggerated, it is possible to construct a model which by far surpasses the releasing effect of the natural object. In the oyster-catcher, for instance, the I.R.M. which responds to the egg and elicits incubation activities is dependent, among other key stimuli, on the size, the colour, and the contrasting spots of the egg. A real oyster-catcher egg is the same size as a bantam's, its colour is bluish grey with slightly darker grey spots. If one presents an incubating oyster-catcher with an egg nearly as large as an ostrich's, of bright blue colour and with deep black spots, thus exaggerating the intensity of the key stimuli mentioned, the bird becomes absolutely fascinated, leaves its own clutch and passionately tries to incubate the giant egg, though this is physically impossible, the bird being hardly able even to stand astride the model. A very intelligent American journalist, on seeing Tinbergen's film showing

this behaviour, exclaimed: 'Why, that's the cover-girl!' This witty remark is scientifically quite correct. Much of what we call 'vice' in human behaviour does not consist in anything else but the search for supra-normal key stimuli. The vice of gluttony offers very convincing examples, and quite proper ones at that.

Imprinting

There is one particular type of I.R.M. whose function is closely linked with a particular type of conditioning. I think it may be of interest to you, because this interaction between I.R.M. and learning is limited to a strictly defined phase in the organism's ontogenetic development. These I.R.M.s are of an extreme simplicity and therefore their selectivity is slight. But this lack of selectivity is compensated by the limitation of the time during which the I.R.M. is effective. During that short period, the I.R.M. succeeds, under natural circumstances, in establishing a conditioned response to its object. The resulting response then is far more selective than any I.R.M., as all learned responses always are. Let me give an example. The I.R.M.s eliciting, in a newly hatched greylag gosling, the activity of following its mother respond to an amazingly wide range of key stimuli. Any object between the sizes of a bantam hen and a big row-boat, which moves and emits noises of a wide variation of pitch, can release the following response of a newborn greylag. If a man moves and talks in the presence of such a little gosling, the latter will look at him very intently, give its greeting response, and, after a few repetitions, follow him unconditionally, just as it would normally follow its mother. Obviously this combination of a simple I.R.M. and consequent conditioning is entirely effective under natural conditions. The cases in which the mother goose is not the first moving and sound-emitting object perceived by the gosling are, of course, so extremely rare as to be of no account.

Now the particular kind of conditioning that takes place in the process just described differs from other types of learning in a number of very characteristic points which I want to summarize:

- (a) It is limited to a very definite and often extremely short phase of ontogeny.
- (b) It is, in the typical cases, quite irreversible.
- (c) It takes place quite independently of whether the activity released by the stimulus situation is, at the time being, functional or not.

Mainly because of its irreversibility we have called this particular type of conditioning *imprinting*. The last of these three points is very important, because you cannot get any other conditioned response when the unconditioned response is not yet functional. That is what

makes us believe that imprinting is something which takes place in the perceptive sector only, because the efferent sector of the reaction-arc need not be present at all.

There are two points about imprinting which seem important in this connexion. First I want to call your attention to a very enigmatic fact. The gosling in the classical imprinting experiment of Heinroth does not become imprinted to the particular man whom it sees at first, but to Man, with a capital, as a species. It can learn to know its keeper, later on, by a common learning process, but the irreversible imprint refers to the species and not the individual. We have no explanation of this at all.

Secondly, I want to emphasize that most probably there are all imaginable types of conditioning, forming gradations between true imprinting and the more common types of learning. What we call imprinting is a type we found at first in jackdaws and greylag geese. It is one extreme type of learning, and there may be all sorts of gradations. We know already from the work of Eckhardt Hess that, for instance, depth perception is acquired in a way very similar to imprinting, and if you prevent a chick from acquiring it during the first days, it cannot do so afterwards. That is not a fixation of a simple activity to its objects, but it is like imprinting in being confined to a very short phase of the ontogenetic development of the individual. I mention this only in order to emphasize that there may be all sorts of gradations; that is one example of extreme imprinting in some respects and not in others. There may be a superposition and interlocking of imprinting and learning. Such intercalations, though superficially tending to veil the irreversibility of imprinting, really afford its most convincing proof. The budgerigar, to give you a good example of this, is a bird which learns easily to accept substitute objects for its sexual activities. You have probably seen budgerigars in cages court a celluloid doll, etc. We imprinted the sexual activities of two budgerigars, a male and a female, to the human species—which it is quite easy to do by rearing the bird in isolation during a certain period. Then we deprived them of all human company and kept them together in a lonely garret where they were fed and watered through chutes. After a time they learned to use each other for substitute objects, courted and copulated quite normally and even reared two broods with full success. Now after the third brood had hatched, we did the crucial experiment which simply consisted in my going into that garret. Both birds instantly went into a frenzy of sexual excitement which was directed entirely towards my person. The impression of this reunion with what the birds ‘considered’ their own species was so lasting that they absolutely refused to have anything to do with each other for a long time and let their brood of

young die of hunger. Now consider that these birds had many times successfully copulated with each other, while all their attempts to copulate with the imprinted object of their passion necessarily had always remained unsuccessful and unsatisfying. Nevertheless they persisted in their object-fixation, if I may call it thus! Though imprinting has been found in its typical form in birds and insects rather than in mammals, I really do believe it to be fundamentally akin to those very lasting object-fixations of human beings, chiefly because these fixations also seem to be dependent on early childhood impressions and seem also to be largely irreversible. Some psychiatrists and psychoanalysts here I believe share this opinion, at least as a working hypothesis.

I now will proceed to show you a short film illustrating some of my points. The first part of the film shows the behaviour of a flock of young goslings imprinted to man, in comparison with another flock, imprinted to its own species, and the second part shows reproductive activity.

Dr. Lorenz' film illustrated the following response, reaction to warning call, unmixing of the two differently imprinted flocks, following response on the wing, greeting reaction, pair formation, copulation activities, and neck-dipping.

RÉMOND :

Dr. Lorenz started his introduction by speaking of reflexes and seemed to say that actually reflexes do not exist. . . .

LORENZ :

No, excuse me. I said that the reflex is a special case of response of the central nervous system, which exists in its classical form in certain animals capable of a high degree of differentiation.

RÉMOND :

How would you call the retracting movement of an oyster when touched with the prongs of a fork?

LORENZ :

That is just the point: I would not give it a name at all, before knowing what it is physiologically! All functions of the central nervous system ought to be termed in a way containing as little hypothesis as possible. The thing to do is to give a purely descriptive term corresponding to what we know about incoming stimulation

and outgoing activity. In your example of the oyster the input is very probably a single wave of excitation and the output consists in a long-lasting increase of muscular tonus—most muscular contractions of molluscs are tonic. This physiological process is certainly 'a reaction', but very different indeed from a 'reflex'. This word always calls up the mental picture of the classic diagram of the cross section of the cord with the short reflex arc coming in at the posterior root and going out by the anterior root.

RÉMOND :

Then you limit the definition of reflex to this set-up?

LORENZ :

More exactly to the function of this set-up, which consists of a single wave of efferent excitation responding to a single wave of afferent excitation. This is certainly a reflex, but it is very rare in animal behaviour. The only examples I can give are those I have already quoted. Autorhythmia, as in the creeping movement of the earthworm, is as rare as the other, and between these two extremes there are thousands of kinds of function of the nervous system.

RÉMOND :

You spoke of autorhythmia as if it were exceptional, but is not this property in fact one of the most general of the nervous system, at least from the electroencephalographic point of view? It has been found in the most elementary structures. Thus, it was demonstrated long ago by ADRIAN (1931) in the optic ganglion of the *Dysticus marginalis*. He was able to show that even when isolated this ganglion was in continuous activity. When it is connected to the eye, or what replaces the eye in this animal, this activity ceases immediately light arrives. Autorhythmia is a kind of negative to activity.

GREY WALTER :

It is the lack of electrical activity, of physiological activity, isn't it?

RÉMOND :

Certainly. That is exactly what I was coming to.

LORENZ :

I actually had intended to cite the sensory cells of the mucous membrane of the olfactory tract as an example, whose particular

function also is something extremely queer and rare. When the output of the central nervous system is arrhythmic but continues independently of afferent excitation, this function is termed 'autostaxis'. This autostaxis of the olfactory sensory cells can change into different autorhythmias, according to the smell which impinges on it. The moment a smell impinges on these olfactory cells, they begin to fire rhythmically in variable rhythms and each of these different rhythms is characteristic of one particular smell. Adrian can look at the curve of electrical activity given out by those cells and is able to say what kind of smell is impinging on the mucous membrane of the rabbit (ADRIAN and LUDWIG, 1938). That is an immense achievement. Of course, the reflex on the one hand, and the autostaxis and autorhythmias on the other hand, are joined to each other by a number of gradations: for instance, my pupil PRECHTL (1952b) showed that the gripping reflex of the human child is apt to be a rhythmic movement in the first postnatal period. After stimulation it occurs several times. Only with the growth of the afferent control, the unnecessary and even detrimental sequence of movements is cut out and one single movement is left. The same is true of what PRECHTL and SCHLEIDT (1951) call the 'search automatisms' in young mammals and human babies. These have an autorhythmic movement of searching for the teat by turning their head to and fro. Now if you analyse this function, you find a very exceptional connexion between an automatism and an I.R.M. In the typical cases, the automatism is blocked all the time, until released by an I.R.M. which removes the block. Here, it is the other way round. The searching automatism is going on all the time, as long as the little animal is awake. It only ceases when a very definite stimulus situation is attained. In the cat, the responsible key stimuli emanate from the hairless area round the mother's nipple. As soon as the kitten finds this, its searching automatism is blocked. In other words, the I.R.M. puts on a block, instead of removing it as it does in most other cases. In the kitten, as well as in the human baby, it often happens that the I.R.M. responding to the nipple fails to inhibit the to and fro movement of the head at once, but does so with a slight delay, so that the little creature seems to look for the teat in the wrong place, which gives an effect very similar to the superposition of flight reaction and prehensile reaction which we saw in Dr. Monnier's film.

What is called the 'prehensile reflex' in the human baby, is nothing other than the search automatism brought under a strict control of afferent functions. These release the automatism on a slight touch of the corner of the mouth, allow it just one sideways stroke, and stop it at once when the nipple is found. The ontogenetic development of the 'prehensile reflex' out of the 'search automatism' has been

thoroughly demonstrated by Precht! And yet you may call it a reflex! For very possibly a reflex, even in its purest form, is nothing but an autorhythmic process brought under the rein of afferent control to such an extent that it is just de-blocked, allowed to discharge just one stroke of excitation, and instantly blocked again. That, of course, is only theory. I only want to repeat that I neither assert that 'there is no such thing as a reflex', nor do I believe that autorhythmia 'explains everything', but that there are lots of other types of central nervous functions, just as important as these two, and that it is sheer prejudice to assume that one of these many different performances is 'the' elementary function of the central nervous system.

Now, apropos of the film, allow me just a few words on a curious phenomenon which we found in the reaction of the goslings to our imitation of the mother's warning call. It invariably was strongest at the first experiment and tended to fade very rapidly indeed with each repetition. Attempts to reinforce it were quite unsuccessful. The fading could not be prevented by letting even very strongly frightening stimuli impinge on the goslings immediately after the warning call. And even after a very long period of rest, the former intensity of the reaction does not become re-established. This is one of the chief problems which we are trying to solve at the moment.

RÉMOND:

A similar reaction in man is an absolutely natural and physiological phenomenon which occurs even during sleep. If you take as a reference the existence of a cerebral electric reaction to stimulation during sleep, you find that on repetition of a known stimulation the response very rapidly weakens until it subsides completely. If the nature of the stimulus is modified somewhat the reaction reappears, but it fades away again. On returning to the first stimulation after a certain time, it is again found efficacious. It seems that this is a very general property of the nervous system, which is on the alert only for what is new and unexpected; but as soon as there has been some kind of apprenticeship—and this apprenticeship does not necessitate a conscious vigilance—it economizes part or all of the reaction. This does not mean to say that vigilance is lost, since the least modification in stimulus causes reappearance of the reaction.

LORENZ:

I am very glad that Dr. Rémond brought up this question, which I did not make clear. Of course, these are phenomena which the physiologists have called adaptation; a rather unhappy term, because this type of adaptation has nothing to do with adaptation in the biological

sense. Processes of physiological 'adaptation' may be biologically adaptive, or they may not. In order to make clear what I meant by special cases of 'adaptation', I had better describe the experiment of ROBERT HINDE (1953). Hinde found an I.R.M., whose effect he could quantify very well, the mobbing activity of the chaffinch. Chaffinches, like other small birds, react to owls by giving their warning call, approaching the owl to a certain distance, and then following it about uttering their warning calls all the time, executing certain movements of characteristic excitement, actually displacement activities. The activity goes on for a measurable time, after which they quieten down. Hinde originally wanted to analyse the I.R.M. eliciting the mobbing reaction of the chaffinch, but what he discovered was something much more important. The reaction depended on a very simple set of key stimuli, as most I.R.M.s do. The owl dummy only has to be round, it has to have eyes in a certain place, and so on. Now he quantified the number of sideways movements and the number of 'pink, pink, pink!' elicited by a standard dummy, until the reaction quietened down. The average of the number of sideways movements and the number of 'pinks' he found in virgin birds was taken as 100 per cent.

Then he found that in a second trial on the same day he got about 30 per cent of that, and in a third trial he got about 10 per cent, and then, finally, 1 per cent. After a certain period of quiescence of one or several days he again got about 50 per cent and after a period of quiescence of six months he came up to 62 per cent, and that was all. Now we have no doubt that the reaction of the chaffinch to the owl has survival value. It was very surprising to us that this reaction is devalued by half if this unlucky bird happens to meet an owl once who who isn't hungry and is for several hours in that region. If this is so the reaction loses its biological usefulness altogether. Hinde thought that perhaps reinforcement was necessary. This was already a measure of desperation, because it is obvious that if the reaction was dependent on the bird getting nearly caught or actually pinched by the owl—which would be the only manner of reinforcing it—the reaction would be of little use to the bird. Well, he didn't succeed any more than I did in reinforcing the goslings' reaction to the warning call. We found that quite generally this 'fading' of reactions to I.R.M.s takes place whenever we elicit them with *dummies*. We strongly suspect that there must be something wrong with our experiments. What I propose to do now is this. Next year, I will take one of my greylag geese who is now two years old, and I will incubate and rear the young. Then I shall make the mother-goose warn, which I can easily do by presenting her with one of our eagles, or with a stuffed cat, or stuffed stoat, on a wire. The mother warns her flock twice. I

shall have also a flock imprinted to me. I shall warn my flock exactly the number of times the mother does. I shall go on doing this and then we'll see if there is some difference in the fading between my goslings and those of the mother, because what I suspect is that in some way, with this rather crude model, you supply a key which doesn't quite fit, and in some way spoils the lock.

On the other hand, the phenomenon of adaptation is intriguing us in a very different manner. There is, in the reaction to the key stimuli constituting the I.R.M., a phenomenon of 'adaptation' which is most important. PRECHTL (1953) did a simple experiment with chaffinches in their nest which have a gaping reaction that is very nearly a 'reflex'. It can be elicited (*a*) by the slight trembling of the nest when the parent bird alights, and (*b*) by the call-note of the parent bird. These two stimuli are about equally effective quantitatively. Now you can stimulate gaping by shaking the nest and get about five or six reactions after which the bird doesn't react any more. Then you give the auditory stimulus and get six reactions more. If you invert the sequence of the stimuli, the numbers of responses remain virtually the same, which shows that neither of the stimuli is *a priori* more effective than the other, but that each of them becomes less effective with the number of repetitions. This 'blunting' of the reaction to one particular key stimulus (I prefer the word 'blunting' or, in German 'Abstumpfung' to the ambiguous term 'adaptation') is a phenomenon which we have hitherto neglected. It is a very important source of error in all attempts to quantify the exhaustion of the efferent side, of the instinctive movement itself. Also, it is a source of error in the quantification of the efficacy of the single key stimuli, as compared with each other. One cannot test one key stimulus without blunting the response to it, and therefore any new stimulus, tested after some other, appears to be relatively stronger than it really is. Furthermore, there is one queer thing about this blunting or adaptation which we cannot explain at all: if, in the experiment with the gaping baby chaffinch, the reaction to one of the two key stimuli is totally exhausted before the second stimulus is brought into play, the total of responses elicited by both amounts to an average of eleven. But if, on the other hand, the change of stimuli is effected before the first one has become completely blunted, and then a second change, back to the first stimulus, is made before the second one has become ineffective, and so on, back and forth, a total of forty-eight reactions can be released on the average.

FREMONT-SMITH:

Suppose that the chaffinches who could react to either of two stimuli got them both at once, what would happen then?

LORENZ:

You would get a reaction of high intensity which might last considerably longer than the reaction to one of them. But you get a smaller number of reactions than by changing the stimuli.

GREY WALTER:

In the case of the goslings whose reaction to warning of the owl's presence appears to fade, is there any evidence that if you allow your real or model hawk to catch one of the goslings the fading becomes less? In other words, does the experience of a catastrophe to a flock act as a reinforcement—does that tend to keep the response up or is there no such interaction?

LORENZ:

We are not quite able to answer this. We did rather mild and not very thorough experiments by just letting the hawk swoop a bit and we didn't get any influence from that. Prechtl didn't get any influence by feeding his birds after gaping, which would have been a reinforcement. I think that when an instinctive movement does lead up to a definite consummatory act, then the consummatory act tends to act as a quite definite reinforcement; there is no instance of fading in that type of I.R.M. Fading only occurs in another type of instinctive activity, in which there is no definite consummatory act.

Dr. Rémond spoke about a little change in the stimulus allowing the full reaction to occur again. In this regard SEITZ (1940) could get the whole courtship activity of the male astatotilapia, up to fertilizing the egg, with dummy experiments. (The last step is elicited by olfactory stimulation emanating from the eggs, and he couldn't get that because the plasticine dummy didn't lay eggs.) When he painted the dummy a slightly different colour the reaction returned after fading and the curve of response rose quickly, but also was lowered more quickly. After repeated change of dummy the fish learnt that even a new dummy wouldn't be any good. He was 'disillusioned' as to dummies. But this 'disillusionment' is certainly not identical with fading.

FREMONT-SMITH:

Bronk showed, if I remember correctly, that the response of an isolated sympathetic nervous ganglion to one type of input would depend upon whether or not it had received previously a different type of input along the same or along another nerve. One could even say of the isolated sympathetic ganglion that its response to a given

stimulus depended upon its past history. It seems to me that there must be a continuing effect of some sort in these cases also, a residual that has been left in.

GREY WALTER :

The whole nervous system is designed to deal with a complex environment in which what matters is not that something particular occurs but that something is related to many other things. The isolated peripheral nerve fibre is a special case in which this is not true, because the isolated peripheral nerve fibre is purely a communication system, not an analysing system; but the moment you get into the spinal cord, the importance of coincidence and the importance of time-relations between stimuli becomes obvious.

FREMONT-SMITH :

Isn't that also true in the isolated nerve fibre?

GREY WALTER :

It is not quite true, it's rather doubtful, I think. By mutilation you can make a nerve fibre regress to something a little bit more un-specialized; for instance, by cutting the ends and joining two together you can make an artificial synapse, but this is really a very special case of experimental interference.

LORENZ :

I should like to mention the experiment of BIRUKOV (1952), of Freiburg. He tried to exhaust righting reactions in frogs. Righting reactions have a tremendous amount of energy at their disposal and take an immense time to get tired. He found that on a wobbling surface the righting reaction tired after several thousand attempts. When the frog was tired, and didn't right itself any more, a change of axis of tilt of only a few minutes of angle resulted in awakening the reaction again. Only then it got tired sooner, and the smaller the change in angle the sooner it tired.

BOWLBY :

There are two questions I want to ask, the first regarding the notions of 'fading' and 'disillusionment'. Is it true that these are two separate processes? You mentioned the 'fading' that can occur when I.R.M.s are elicited with dummies and you used the analogy of the key that doesn't quite fit and spoils the lock. In the second illustration, when you used the word 'disillusionment', you said that the

consummatory act reinforces the response and that a certain fish learnt that dummies were no good. If you take that fish back to a real female, are its copulatory activities diminished?

LORENZ :

Well, we haven't done that, but to our isolated fish the female would have been nothing but another very new dummy and I should predict that he would react strongly and come to copulation with the female and this ultimate 'success' would reinforce the reactions to it. He would give the reaction again and again with her. But I agree that fading in I.R.M.-elicited activity without consummatory action is something different from the fading which is brought about by non-reinforcement, because in the cases in which the satisfaction of the consummatory act is lacking, the 'extinguishing' of the response is due to true learning.

RÉMOND :

I should like to ask Dr. Lorenz whether the warning reaction given by the mother goose to her young when she sees the eagle depends on experience or whether it is absolutely spontaneous the first time an eagle goes by. And would the effect on the goslings be different if a duck or a goose went by instead of an eagle when the warning call is given?

LORENZ :

Well, I am afraid I must answer this question very extensively, in order to prevent generalization. I will answer it for the goose first. In the goose, the reaction to the eagle is purely innate, and the most curious thing is that the little geese look at the eagle when the mother is warning, but do not react to it afterwards on their own account. They do not learn by being warned, nor later remember it. It is not necessary, because they keep together with their mother. If you try to make experiments on a hand-reared goose it does not react to the eagle at all until it is fully fledged, and then it suddenly, at about nine weeks, begins to react to the eagle with an immense intensity, irrespective of whether it has seen eagles before. The I.R.M. of the eagle in the goose is extremely simple. An 'eagle' is anything which (a) is depicted against the sky as a background, (b) does not beat its wings, and (c) moves slowly. A goose will give the full eagle-reaction to a black feather drifting slowly in the breeze, or a pigeon gliding slowly against a strong head wind without beating its wings. You see all the geese giving full eagle-reaction to the pigeon, and when the pigeon gives a few wing-beats all the geese give an immense relief

displacement-shaking and are quiet again. One funny thing takes place, which is just the opposite of imprinting. My first goose, Martina, had no experience of being warned by parents and didn't react to aeroplanes which were flying there all the time. She did not seem to notice the aeroplanes until her I.R.M. was mature, and then she suddenly got awfully afraid of them. But after a time she quietened down again, and got adapted to the aeroplanes flying over her.

Now let's take another bird, the jackdaw. It has no innate reaction to enemies at all. One single case of hearing the warning call of the mother forms the full association between the object at which the mother warns and the reaction. Again, in a turkey, the I.R.M. of the bird of prey is much more complicated: form plays a role in it. If you make a dummy that has a short process on one side and a long process on the other side, and you draw the dummy across the sky on a string with the short process pointing forward, then the reaction is the reaction to the hawk, and if you point the long process forward, then it is a goose, and there is no reaction because a goose is not dangerous. KRÄTZIG (1940) showed that grouse have two different reactions to two different birds of prey. They reacted to an eagle dummy by taking to wing and towering, and they reacted to a falcon dummy by taking cover. Krätzig has been reproved for doing too few experiments, but the whole point is that you cannot do many experiments because you get a conditioned reaction very quickly. When Tinbergen and I were experimenting on geese, one day, I came into the garden; Tinbergen had prepared our dummies, and as I came into the garden he said, 'We aren't going to experiment today'. I said, 'Why not?' He said, 'I'll show you'. In order to attach our dummies to the piece of metal that slid on the wire it was necessary to climb the tree, and Tinbergen and I used to climb it by throwing our legs over a certain branch. Tinbergen went to quite another tree and threw his leg over its lowest branch—and all the geese looked up to the sky and went to cover. And that is why for I.R.M. experiments you ought to have a new animal for each trial.

MEAD :

Dr. Lorenz, you mentioned three conditions under which diminution of I.R.M.s might occur: hybridization, domestication, and ill-health. Can you differentiate among them in the sorts of damage done? It seems to be conceivable that ill-health might diminish the strength of the response to each of a series of stimuli, and that you might get a quantitative but non-selective diminution, whereas in another case the diminution might be selective. I will give you an illustration of what I mean. In Bali I was in a village which turned out to be an

area of acute hypothyroidism; every single aspect of the culture had been simplified, but without loss of pattern. Instead of putting a hundred items in an offering they put twenty-five in, but they kept the essential elements—showing the difference between the diminution that you get with hypothyroidism and that which you get with neuroses.

LORENZ:

The question is very fascinating and I do not know whether I can answer it out of hand. There is in the reaction to the I.R.M. a loss of selectivity and that may result in making the release of the reaction easier.

MEAD:

A kind of vulgarization?

LORENZ:

Exactly! This would result in a person being sooner content with an object. That object need only possess a few of the releasing characters which act as key stimuli to the normal, undisintegrated I.R.M., and a lot of others may simply be dropped out without impairing the object's releasing value. And you have no idea how very aptly the word you have just chosen, vulgarization, describes the situation. If you compare, for instance, the sexual behaviour of a wild greylag with that of a domestic goose in which the whole elaborate courtship ceremonies have become irrelevant, and in which any goose and any gander that are put together during the mating season will proceed to copulate without any more ado, you cannot help feeling that the behaviour of the barnyard goose is grossly vulgar. But I'd better tell you, in some more detail, the sequence of the disintegration of an I.R.M. in domestic hens which Heinroth studied extensively.

He found that the first key-stimuli which became independent were those of colouring. In the usual domestic hen that you find with peasants, a hybrid race of fowl, he found no hen that minded whether her chicks were black or white. In Phoenix hens, the long-tailed type, he found a good proportion of hens that would kill black chicks, and did not mind white chicks. In bankivas he found hens that would kill everything that was not wild colour. My friend Otto Koenig, in Vienna, tried to repeat those experiments with bankiva hens of rather doubtful origin to see if those things drop out in a few generations—we do not know how fast domestication occurs—and he did not find a single one that would kill a black chick, whereas there is no doubt

that Heinroth's did kill black chicks. (A nice point is that they do not mind and are quite ready to mate with a black cock.) You find barnyard hens who will refuse to brood ducks and even kill the duck in the egg when they hear the first cheeps. Yet Orpingtons and Rhode Islands will take goslings and ducklings, so long as they are downy: they won't take a naked bird. If you let them hatch a raven or a cormorant, which are about the largest birds whose newly-hatched young are quite naked, they will kill them. Then there are Brahmans that will brood anything up to kittens. It's a nice sequence, isn't it, how the characters drop out singly.

TANNER:

All these things Dr. Lorenz has been describing make one think that there are circumscribed mechanisms in the central nervous system which should be relatively easily susceptible to anatomical and physiological delimitation. Has, in fact, any work of this sort been done? Presumably there are centres where the energy for the I.R.M.s builds up. Where are they?

LORENZ:

I am not quite in agreement with the hypothesis that they must be circumscribed, for one reason: you must keep in mind that where reaction to form is concerned, as, for instance, in the turkey cock, the whole thing must somehow go through a Gestalt perception, an apparatus certainly very widely distributed inside the central nervous system. That does not apply to all I.R.M.s and, of course, there are all gradations including the case where one sensory organ only is required to elicit one response. If you have a cricket, where the female has a hearing apparatus with a range of only a few tones in pitch, and which is only meant to receive the mating call of the male (which releases a positive reaction causing it actually to jump into the loud-speaker in the celebrated experiment) there the question of selectivity does not arise; but when the jackdaw reacts in one way with a submissive attitude and in another way with an aggressive attitude, then the whole thing must be shunted somewhere over the remotest projections of the cortical visual apparatus.

TANNER:

I was thinking more of the effector end of the mechanism. After the Gestalt has been received, brought in over certainly many pathways, one would imagine that these then converge on some distinct anatomical structure which would be the power structure of the I.R.M.

LORENZ:

Some results of Professor Hess tempt one to think that in his excitation experiments he gets a place at least very near to the input on the receptor side (HESS and BRÜGGER, 1943). In most of Professor Hess's experiments he got an entirely coherent, integrated sort of behaviour. If he stimulated the 'fighting centre', the cat behaved entirely as if there really was a rival. In such cases one would suspect that the excitation influenced a centre very high up in the hierarchy of the central nervous system, or even that it was the afferent side of the response that was stimulated. Such a cat behaves exactly as if it had an hallucination of another cat fighting it. But the interesting point is that Hess could get different levels of the hierarchic system of this instinct at will. When he let a very slight stimulation impinge at the more cranial point, he got a slight threshold-lowering of fighting-reactions. The cat would not yet attack *in vacuo*, but it would bite Hess's assistant whom it would not have bitten without that stimulation. In other words, slight stimulation at that more central point would put the cat into 'fighting mood'; if the stimulus was increased the cat would attack substitute objects which were still less similar to a rival cat; and so on, up to 'explosive' fighting *in vacuo*. But if Hess stimulated at a point situated a few millimetres farther to the caudal end of the brain, the cat, as a whole, was not put into a fighting mood. It got no threshold-lowering, went on purring peacefully, allowed itself to be scratched by the assistant, and then, on stronger stimulation, it would quite suddenly discharge disjointed fighting movement, like scratching and spitting, only to go on purring in the next moment. All this fits in with Tinbergen's theory of the hierarchic organization of instinct so beautifully that it is almost too good to be true (TINBERGEN, 1951). If you ask me where to look for the localization of an I.R.M., I should say: the I.R.M. joins on to the effector side of the response in those places where Professor Hess got his most generalized instinctive behaviour patterns.

HARGREAVES:

We need, I think, to know whether there are any characteristic differences between mammals and birds in this field of ethology. We talked of loss of I.R.M.s. I want to know if there is any way of putting them back. That is to say, has anybody created in animals new I.R.M.s that didn't exist before?

LORENZ:

Birds and fishes were the first objects in which we found these things. A systematic study of mammals and insects in this regard is in

its initial stages, but these initial stages allow us to state that it is quite surprising how these elementary mechanisms repeat themselves in animals as widely different as a rodent, a bird, a fish, an insect, and a cephalopod. Thorpe has found imprinting in insects which is absolutely comparable to that to be found in birds; the only thing about which we are still very doubtful is imprinting in mammals. Most mammals, except monkeys and man, are animals with a large olfactory region in their brain, and in these macrosomatic animals it is difficult to experiment because you can't control your stimuli—you don't smell yourself. There is the olfactory type of mammal and the visual type, and that is why the ethology of monkeys and of man is so surprisingly convergent with what we find in fishes, and very unlike that of olfactory animals. Nevertheless, we get more similar reactions than we expected, with the exception of imprinting. There are some instances where sexual activity in ruminating animals is optically released, and in these few cases it seems that true imprinting also occurs. All known observations concern sheep and cattle, but I am sorry to say none of them is very conclusive.

FREMONT-SMITH:

I would like to close, if I might, with an anecdote which perhaps will relate, in a light way, the bird to the child. This observation was made by my son when he was about fourteen, on some cardinal birds. He had a bird-station just at the right photographic distance from our back window, and a young cardinal came to the station and began to eat the seed quite vigorously. Then his father flew down to the bird-station, and immediately this little bird crouched, dropped his crest, lowered all his feathers, put his head up in a begging reaction, and would not eat anything on the bird-station unless fed by the father. When the father flew away, he roused, became himself again, stood up straight. This was repeated several times, and my wife and I also saw it. What I wonder is whether we can say that this was a regression to a dependent attitude in the presence of the father.

LORENZ:

Well, I don't think so. I rather think it is just the overruling of the still stronger feeding reaction over the weaker one. I can duplicate that observation by one on young shrikes, exactly the same individuals on which I did the impaling experiments. These shrikes were fully fledged but were still fed mainly by begging. When there is anyone in the room the shrikes are prone to go on begging for a very long time, even when they are already quite able to eat. Now I went for a motorcycle tour for four days, and during these four days those young

shrikes were left alone by themselves in my room. They fed themselves perfectly and they were absolutely healthy, sleek, and fat when I came back. I had important work to do, and sat at my desk and the shrikes were sitting in their cage begging at me, and I said, 'Confound you, you have shown me that you can eat for four days, I am not going to feed you any more'. In the afternoon I saw that the shrikes became seedy and sad and saw that they hadn't eaten one bit, because their begging reaction actually prevented them from feeding themselves, and they would have died of hunger because I was sitting in the room, though they would thrive perfectly when I wasn't. I think that's the chief explanation, and a more economical one, of the phenomenon.

FIFTH DISCUSSION

Electroencephalographic Development of Children

GREY WALTER:

When a child is born it exchanges a physiological for a social environment. It is about some of the aspects of the child coupled with the social machinery that I want to talk.

I am going to leave out of my remarks nearly all matters of technology. For the most part these can be found in the literature (WALTER, in HILL and PARR, 1950; WALTER, 1953). I am also going to take for granted that the information, such as it is, on adult neurophysiology is accessible, though it may not be known to us all, and I am going to assert where I could, in fact, prove. You will have to take my word for much of what I shall have to say, and possibly in the discussions I can amplify some of my statements.

First of all about the general difficulties of studying children. My wife and I have spent some years examining children from many points of view—electrophysiological, psychological, ethological—and we have constantly come up against the difficulty of assessing the nature of the population we are studying. Is it, or is it not, a select population? If one is going to study normal children of school or pre-school age, these children are necessarily drawn from schools, families, and friends who are willing for them to be studied; and that eliminates children whose parents are not willing—parents who have, perhaps, some quite natural superstitious distrust of science and scientists, and particularly of neurological institutes and mental hospitals. This eliminates a certain group of the population so that, when I tell you that this is a normal child or these are normal children, you must weight what I say with that previous knowledge. These are children with no neurological or psychiatric complaints, but they are inevitably selected for their willingness or their parents' willingness for them to be studied.

The next difficulty which we have in considering children is that

development of the individual physiology and psychology is coupled reciprocally with social influences. For example, many delinquent children come from unhappy and broken homes, but if you inspect those homes in detail you find that they also contain genetically pathological specimens. There are brothers or sisters who are mentally affected or insane, there is a father who is alcoholic, a mother who is a prostitute. The separation of social family factors from genetical and ontogenetic ones at the present time I believe to be impossible.

Then, a question of pure technology which I must mention. I hope that many of you who are not physiologists or electroencephalographers will be tempted to read encephalographic and electrophysiological literature, but I do beg you in reading it to be extremely sceptical of the results, particularly from the technical standpoint; to bear in mind that the sort of information we collect from our machines is very liable to deceive us, that only the most refined, sophisticated and flexible techniques of recording and interpreting are likely to be of value. If one goes through the literature on child electroencephalography, one finds very little that, in the last analysis, one can accept as fact. The methods of recording, the methods of display, the methods of transformation, of analysis, of statistical checks—all these are subject to very serious criticism, so that the assertions I am going to make about infantile and juvenile physiology are based mainly on my own work, merely because I do know at least exactly what the limits of accuracy and interpretation of that are. Beyond that, I would not like to go. The accuracy of even the simplest experiments, as you will see in a moment, is very limited.

As you examine younger and younger children, the first feature which you find is a gradual increase in the general amplitude and profusion of electrical activity, and at the same time a progressive decrease in its frequency. The electrical waves from the head, recorded in the conventional manner, get larger and larger and slower and slower as you go down the age-scale. If you put electrodes on the belly of a pregnant woman, you can record before birth the electrical activities of the foetal brain, and these bear out the extrapolated child data: the brain activity of the foetus is, on the whole, very slow, irregular, and poorly synchronized. It can only be observed in snatches, because the foetus is always moving, so that there will be a few seconds when you get nice clean records, then suddenly there will be a sort of convulsion, the child will turn over and the head will have gone.

If we follow our child through its first months of life, we find the slow activity, swelling, waxing, waning, and in some stages of sleep and repose giving place to certain features well known in the adult,

the spindles, 14 c/s oscillations characteristic of adult sleep. One then begins to notice what to me is one of the most fascinating features in the whole of this work, the enormous differences between individuals still in the normal range, a range so wide that I myself hesitate to be in any way dogmatic about what the normal E.E.G. is. If one looks for the meaning of these slow rhythms in children one sees that they are associated with the search for peace, if you like to call it that—the repose and inactivity of the child. As the child begins to get older, spends longer periods awake, pays more attention to its surroundings, makes apparently volitional movements and gives overt expression to affective states, one observes a decline in the slow rhythms. Often one hemisphere will start first to show suppression of this activity—sometimes it is the left, sometimes the right. At a very early age, sometimes as young as two or three months, long periods occur during which the slow activity is minimal, particularly when the child is attentive and being played with by its mother or performing some relatively complicated act.

Now I am going to follow the various E.E.G. components up through the years and discuss what they seem to mean—supposing the components can be considered as individual phenomena. (It is quite possible, of course, that we are dealing with a number of phenomena which have only a superficial resemblance.) Imagine us taking records longitudinally in children and following the development of the slow activity through to adult life. As the child gets older these slow rhythms—the *delta activity*—become more intermittent and in general they decline to a small figure of scarcely perceptible size some time between the second and tenth year. The situation is that illustrated in Fig. 15. At the top of the figure we have a chart showing the prominence of the various components of the E.E.G.—I use the word ‘prominence’ advisedly: it is a measure of both amplitude and abundance of activity together as a product. The data have been obtained by automatic analysis of many hundreds of records; the lines at the top and bottom of the delta band show the range of individual variations. If one plots from 100 to 75 as being the birth range, the variation becomes so wide at the age of about three that you can have one child showing almost no delta activity, and another showing as much as a newborn.

Exceptionally this delta activity can persist until the age of eighteen or twenty. We have made a special study of this type of activity in delinquent children compared with normal children (HODGE, WALTER and WALTER, 1953), and we have been bold enough to suggest a term to describe the psychological features which statistically and experimentally are associated with the presence in the brain of these diffuse, rather poorly synchronized, slow rhythms. This term

is *ductility*, which means, of course, the ability to be drawn out without breaking, for the personality to be deformed and moulded into shape without cracking or taking a permanent set. Ductility was observed first in delinquent children, where we found the presence of slow activity associated with something that shocked me as a physiologist. It was not associated with childishness in the general sense, or with stupidity or intelligence, but with a relatively *good* attitude as judged by psychiatrists and schoolmasters—a good attitude to the children's mother, to their fellow schoolchildren and to their leisure. This was discovered by statistical analysis.

As a physiologist I found this notion of a physiological basis for affection and spare-time occupations almost too good to be true. I was brought up in a very sceptical school of physiology where the study of the brain was considered rather disreputable, and normal psychological functions as completely outside the field of physiological investigation. But here one has a very firm statistical association between a measurable, quantitative, physiological phenomenon and a very complex and a very sentimental aspect of the child's life: the relation to his mother, to his spare time, and to his fellows. If the attitude were bad, if he were a naughty boy or a backward boy, then one would have accepted the relation as a phenomenon of immaturity in the nervous system, or inefficiency of brain metabolism, or what you like. But these slow rhythm boys were the boys who had a *good* attitude, they were the nice boys, docile, manageable, easily-led; in other words, ductile children. In the delinquent population they were the sheep who followed the leader. We have tragic evidence that children with this character very easily find themselves inextricably stuck once they get into a criminal society. They may be pushed in any direction, and naturally tend to drift down the social gradients. The consequences of certain mixtures of different types of this sort can be disastrous. Perhaps one of the most useful things we can do is to see how this work can be applied to such serious social problems as crime and delinquency.

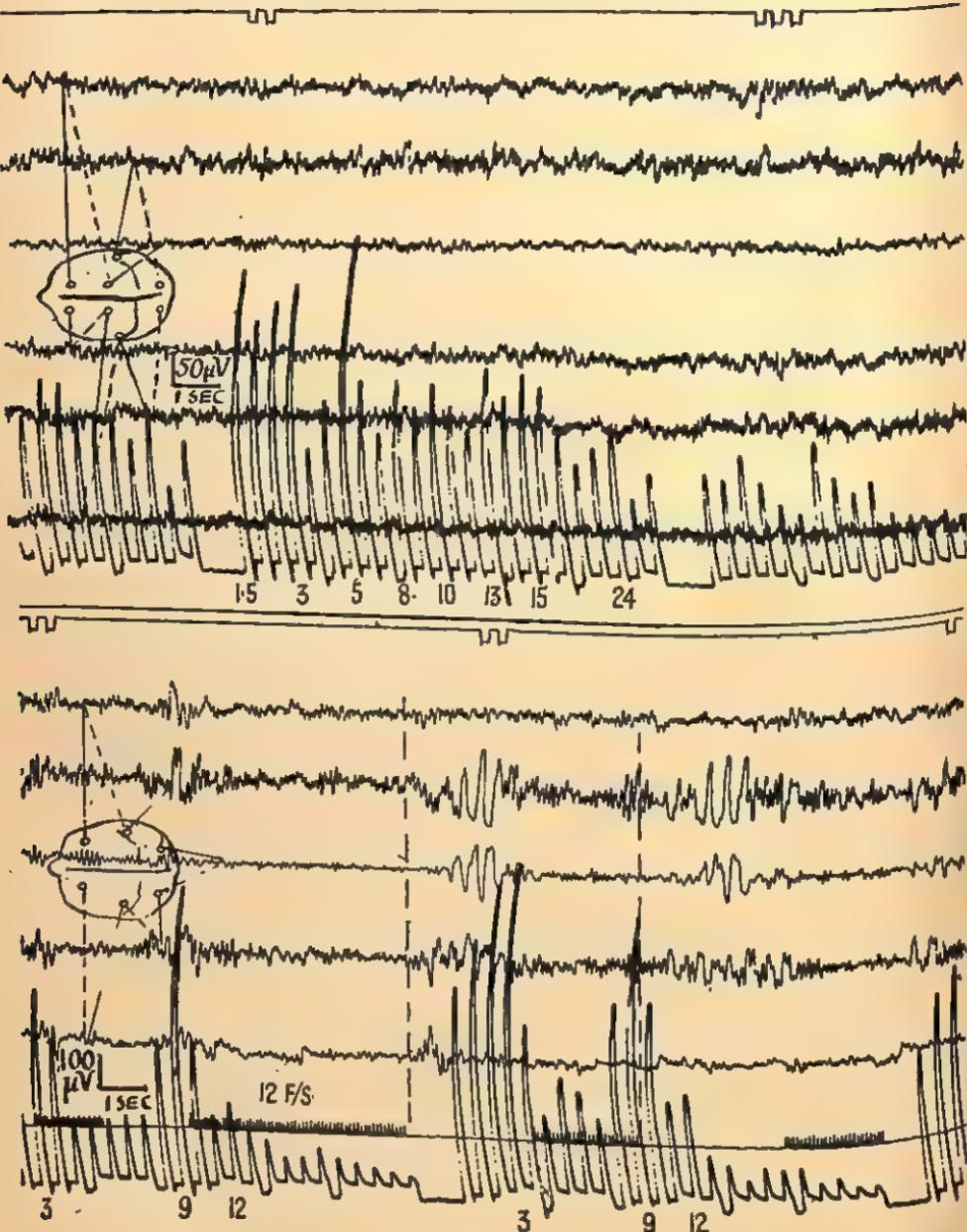
Slow electrical activity is common in adults in sleep, and I should like to put to you the hypothesis that the presence of slow electrical activity of diffuse nature (not associated, of course, with organic disturbance) is the external objective representation of mechanisms in the brain which are directed towards defence of the brain. We all know that in the brain the three mechanisms which in the rest of the body defend against the consequences of injury are totally lacking. If you break your arm you feel pain; as a result of it the arm is immobilized and, even without the help of the surgeon, a better chance is obtained of the bone healing. In the brain you feel no pain. Secondly, you have no lymphatic drainage in the brain to drain away infected

organisms or damaged cells. Thirdly, there is no regeneration of tissue; the brain-cell once destroyed is never replaced. So the brain is in a vulnerable condition. It is enclosed in a hard box, but once that box is penetrated, or something happens in the brain system which is dangerous or undesirable, there are apparently no defences. I would suggest that the slow rhythms represent the one defence. The brain has either to shut off from excessive action a part of itself that is damaged or, in a normal person, to rock to rest those mechanisms of the central nervous function which are either exhausted or have attained a wrong set in the adaptive process. One can show that any complicated mechanism which has its design not predetermined, but which determines for itself its own design, must have built into it quite elaborate mechanisms for what we call 'failure-to-safety': that is, arrangements which ensure that should something go wrong, should some undesirable state occur, the mechanism shuts down as a whole and does not go too far wrong. I suggest that these slow rhythms are a sign of this functioning, as a 'failure-to-safety', of some of the brain mechanisms.

The methods of studying these slow rhythms, particularly in children, from say two to twelve years old, are very important technically because the waves can be evoked and suppressed. Fig. 16 shows two records taken from children both aged twelve. The upper one is an example of a record from an entirely normal child. The presence of the slow activity shows in the record, and the analysis peaks indicate the total quantity of slow activity during each period of ten seconds. The lower record is from another perfectly healthy, happy, normal child, but he displays in the periods of rest large regular slow rhythms at about 3 c/s, as well as an alpha rhythm at 9 c/s. Whenever that child is stimulated by anything whatever, by sound, by a word, or even by an idea, the slow activity is suppressed. During this particular recording a flickering light was intermittently turned on, as shown in the bottom trace. Whenever the child was stimulated in this way, the slow activity ceased. This is a particularly old child to show such very rhythmic slow activity, and he was in fact very highly ductile. He was a charming child; any suggestion you made he would fall in with, any advice you gave him would be taken. His intelligence was quite adequate to take care of any normal problems, but he was easily led, and easily led astray.

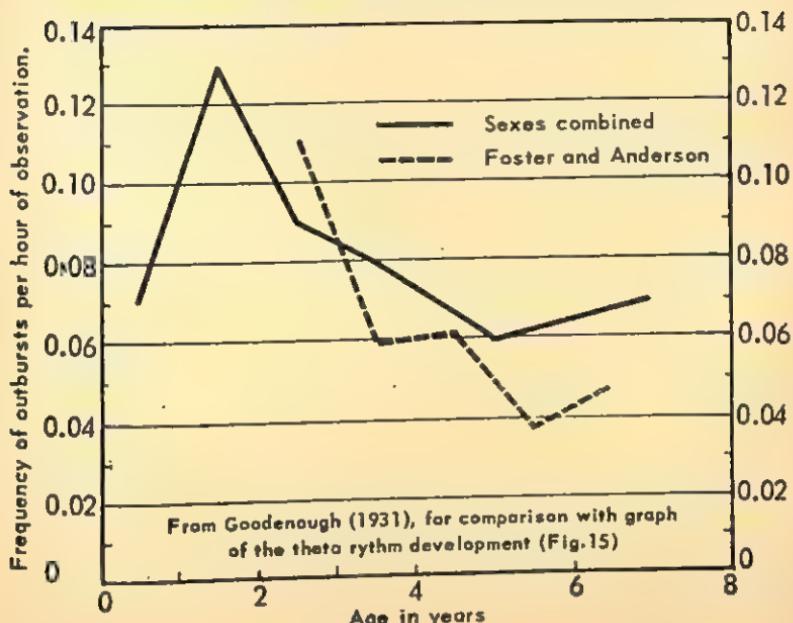
The next feature of children's brain rhythms which seems to be particularly important is the appearance and significance of the *theta rhythms*. These rhythms have a different frequency and a different time-scale. They are sometimes easily seen in the record, but sometimes they are variable and subtle, and apt to be mixed up with other rhythms. In Fig. 15 the large band marked theta represents the

FIG. 16
E.E.G. RECORDS FROM TWO CHILDREN BOTH AGED TWELVE



The upper record shows a characteristic but rather juvenile medley of rhythms in all areas. The lower is an example of an unusually large and responsive delta rhythm at 3 c/s suppressed during the exposures to flicker. This type of record is associated with a high 'ductility'

FIG. 17
NUMBER OF TEMPER OUTBURSTS WITH AGE



growth-range of the theta rhythms in a normal juvenile population. The range is again extremely wide. Fig. 17 is a graph of the frequency of temper outbursts with age in children, taken from the work of GOODENOUGH (1931). There is an astonishing correspondence between the frequency with which temper is lost in children, and the appearance of theta activity. The period of maximum rate of temper outburst coincides not with the maximum height, statistically, of the theta activity, but with the maximum rate of change of theta activity. This period of most likely temper outburst is a period when the theta rhythm is changing most and is the period when it is first taking control of the organism, when it first becomes the most prominent rhythm of all. We have observed this experimentally as well, quite independently. One finds, in fact, that if a child of two or three is happy and content then the record may have little or no theta activity even with the most refined methods of analysis. The moment the child gets annoyed—and, of course, a child can be annoyed by an enormous variety of situations or stimuli which to adults are quite neutral—then one sees a burst of theta activity. This burst may long outlast the stimulus period. It may be a matter of hours, even in some cases of days, before the effect of a certain disagreeable stimulus has

worn off. The first experimental situation in which we observed this was the following: we were taking records from normal children and in the younger children of two or three we found it quite difficult to get them to co-operate well enough for our purposes. We used to give them a sweet on a stick to suck, but for the purposes of the recording itself we had to remove the sweet in order not to confuse the electrical activity of the jaw-muscles with that of the brain. The moment the sweet was removed there was always this burst of theta activity. In the adult we found it more easy to evoke a theta rhythm by withdrawal of a pleasant stimulus than by annoyance. Real deep-seated annoyance is an almost impossible thing to contrive in the laboratory, but you can give somebody a pleasant stimulus and interrupt it, and then in the adult you get a burst of this childish frustration rhythm.

I suggested that the slow delta rhythms represent a search for peace and the equilibration of the organism: I would like to suggest that these theta rhythms represent a search for pleasure as a specific entity. This should, I feel, link up in some way with a psychiatric and psychoanalytic approach to pleasure and pain.

I should like now to discuss the appearance and meaning of the *alpha rhythms*, shown also in Fig. 15. The distribution of the adult alpha types is first seen at about age nine to eleven; in other words, it is about this age that the distribution of alpha types becomes similar to that seen in the adult. I have to describe the situation in this somewhat roundabout way because of the individual variations. There are people who show no alpha rhythms at all, and there are those in whom the alpha rhythm is persistent throughout almost all their waking life whether their eyes are open or shut, whether they are reading, or anything else. Apparently the type of alpha rhythm relates to the type and vividness and persistence of imagery. If the delta rhythms represent a search for 'peace', and the theta rhythms a search for 'pleasure', I suggest that the alpha rhythms represent a search for 'pattern'. There are certain people who, when they close their eyes and relax, are not at rest. The moment the eyes are shut, a picture-show starts. These pictures—I speak from experience—are not in any sense obsessional, but they are quite vivid and can be turned on or off and manipulated as one chooses. In other people this does not happen. The time at which a juvenile population starts to divide into these types is about the age of nine or ten, which coincides with the time when reading ability increases considerably.

The curves in Fig. 15 are simple statistical displays, garnered from a large population and shaken down by statistical methods into a fairly smooth curve; but if you follow an individual child through these phases you get an entirely different picture. Then there is nothing like a smooth curve. For some months the delta activity may

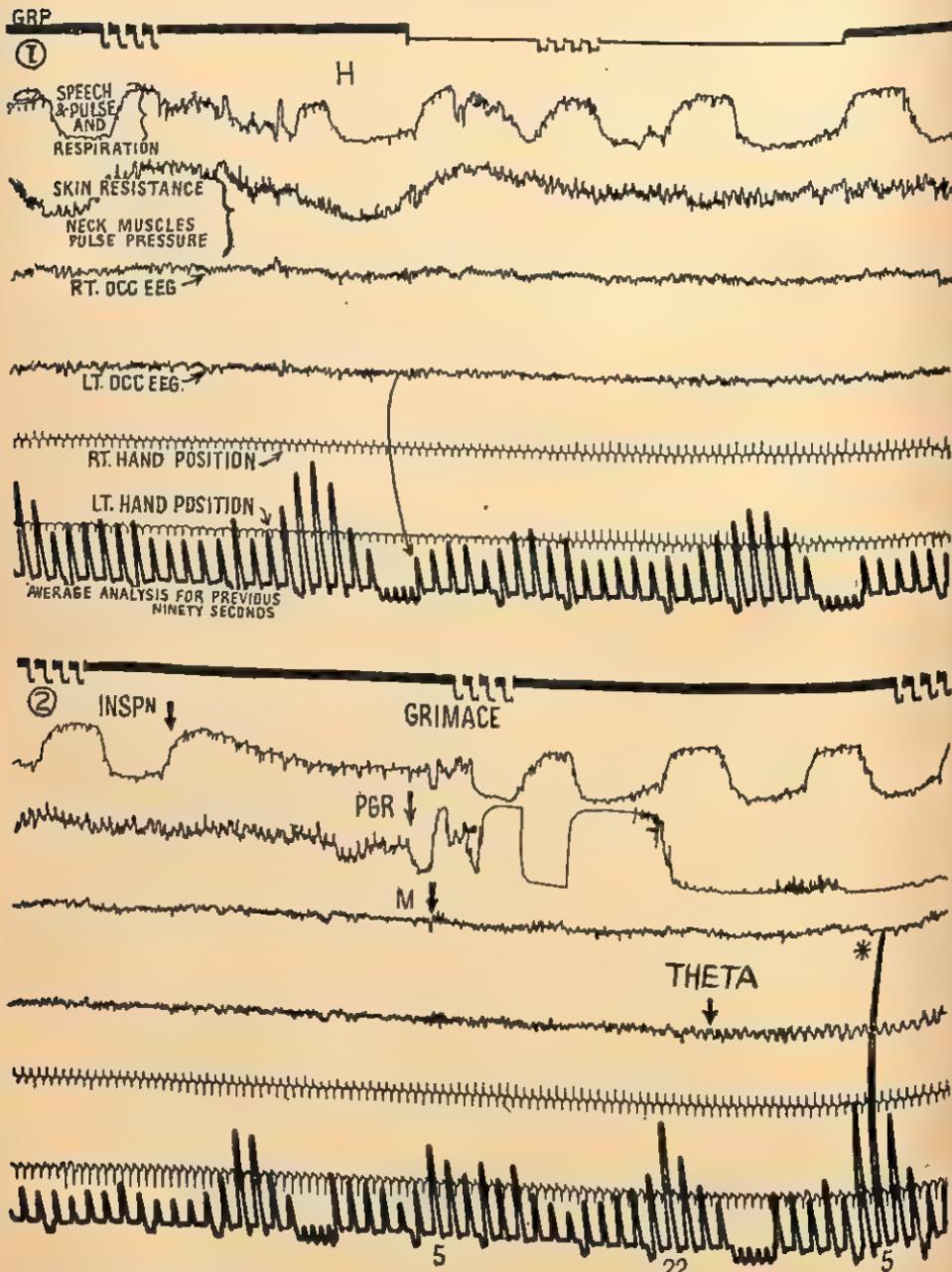
go on showing occasional spindles; then quite suddenly one day you get a burst of theta rhythm as the child wants something but does not cry; the first time the child is deprived, annoyed, frustrated, and does not burst into tears.

The individual records show abrupt changes and also violent vacillations from one world to another, just like a switch which is hovering between contacts and, just while it is closing, chatters for a few days or hours and then shuts irrevocably. This is an observation which, if confirmed and worked out in detail, is very important for general ideas about living animals. The notion of the occurrence in the nervous system of the possibility of abrupt change helps us not only to understand, but to predict, the behaviour of organisms in a way which no other process possibly can. It means that you have a machine which can work within a perfectly well-defined framework of rules and which, if the representative point of that machine gets beyond a certain area, does not break down. There is suddenly a tick-tack-tock and a different set of rules is put up. The brain has a capacity for resetting itself, for setting up its own wiring, which, of course, confounds the Cartesian dualists, who admit that you can make a machine to imitate any property of the human being, but not a machine to imitate the human being itself. I don't say whether you can or whether you can't, but what I do say is that you would have to make a machine that would reset its own contacts according to experience and the chances of survival. Many such machines would fail, but the machines that did survive would be self-adjusting machines, machines which could re-orientate their internal connexions according to what had been found satisfactory on a statistical basis. Such machines would not be logical computers, but computers of similarities and differences, of relations of patterns of environment and behaviour, readjusting themselves according to what had been found to work.

Fig. 18A is a record which demonstrates some of the features I have described. It is from a girl of sixteen who was taking part in a test situation which involved her feeling, blindfolded, with her left forefinger the outline of a groove in a block of plaster, which was in the form of a large letter H. With her right hand she was supposed to draw just what she felt, whether she recognized the design or not. On the top line of the record is her speech, her pulse, and her breathing: on the second line the resistance of her skin, the activity of her neck muscles, and the pressure of her pulse: on the third line the E.E.G. from the right and left occipital regions and then the position of the right and left hands. At the point H the H-block was inserted and she immediately began to feel and explore the groove, and began, rather badly, to draw the outline with her right hand. The

FIG. 18A

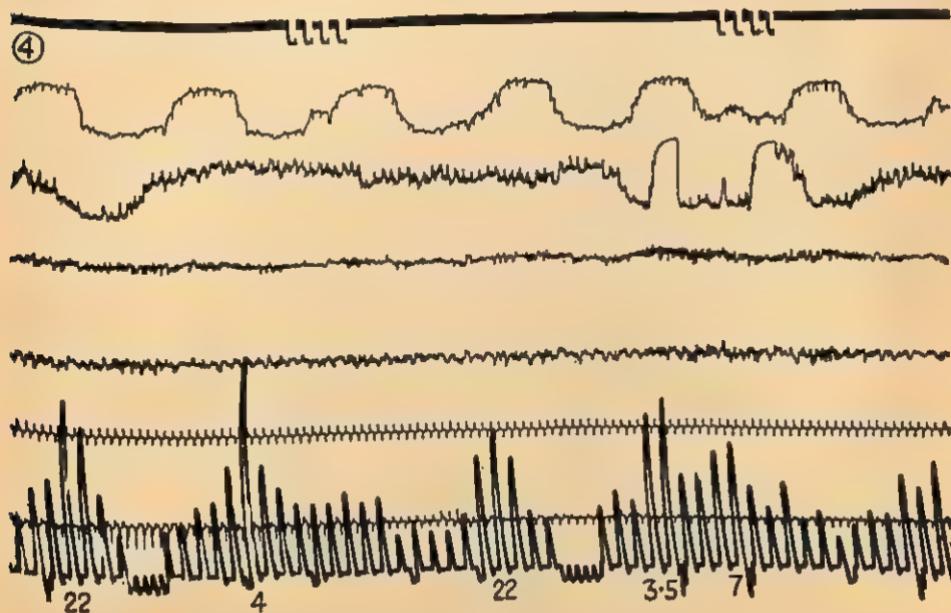
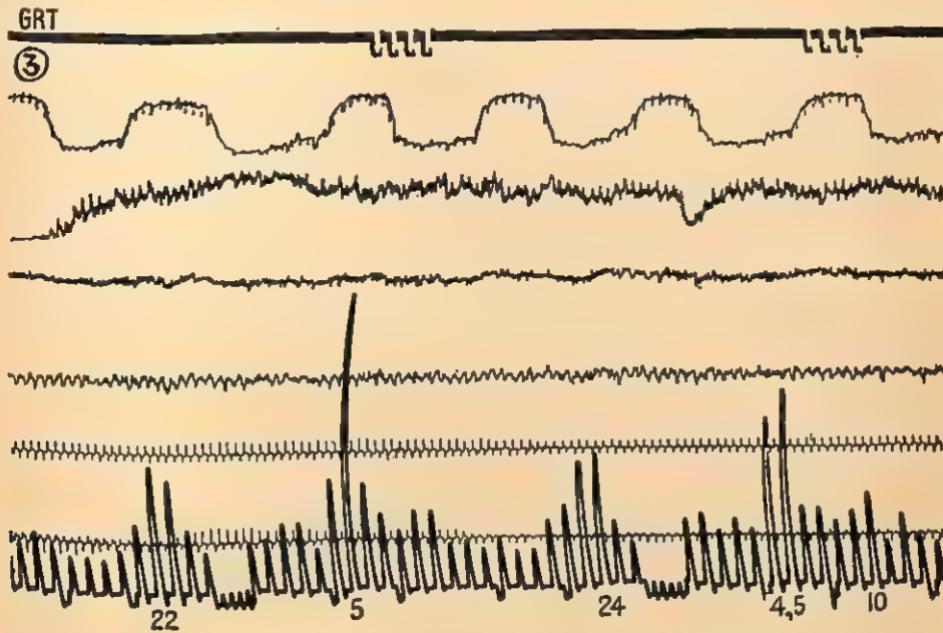
RECORD OF TEN VARIABLES DURING THE PERFORMANCE
WITH THE EYES SHUT OF A STEREOGNOSTIC TASK BY A
GIRL OF 16



The first record covers the start of the task and shows rather irregular breathing and complete suppression of alpha activity. The second record is a continuation of the first and shows a sudden sigh, a grimace, a violent drop in skin resistance, acceleration of the pulse, tensing of the neck and then a sudden increase in theta activity, all associated with a feeling of annoyance and frustration.

FIG. 18B

Showing the persistence and final subsidence of the theta activity as breathing,
skin resistance and tension return to normal.

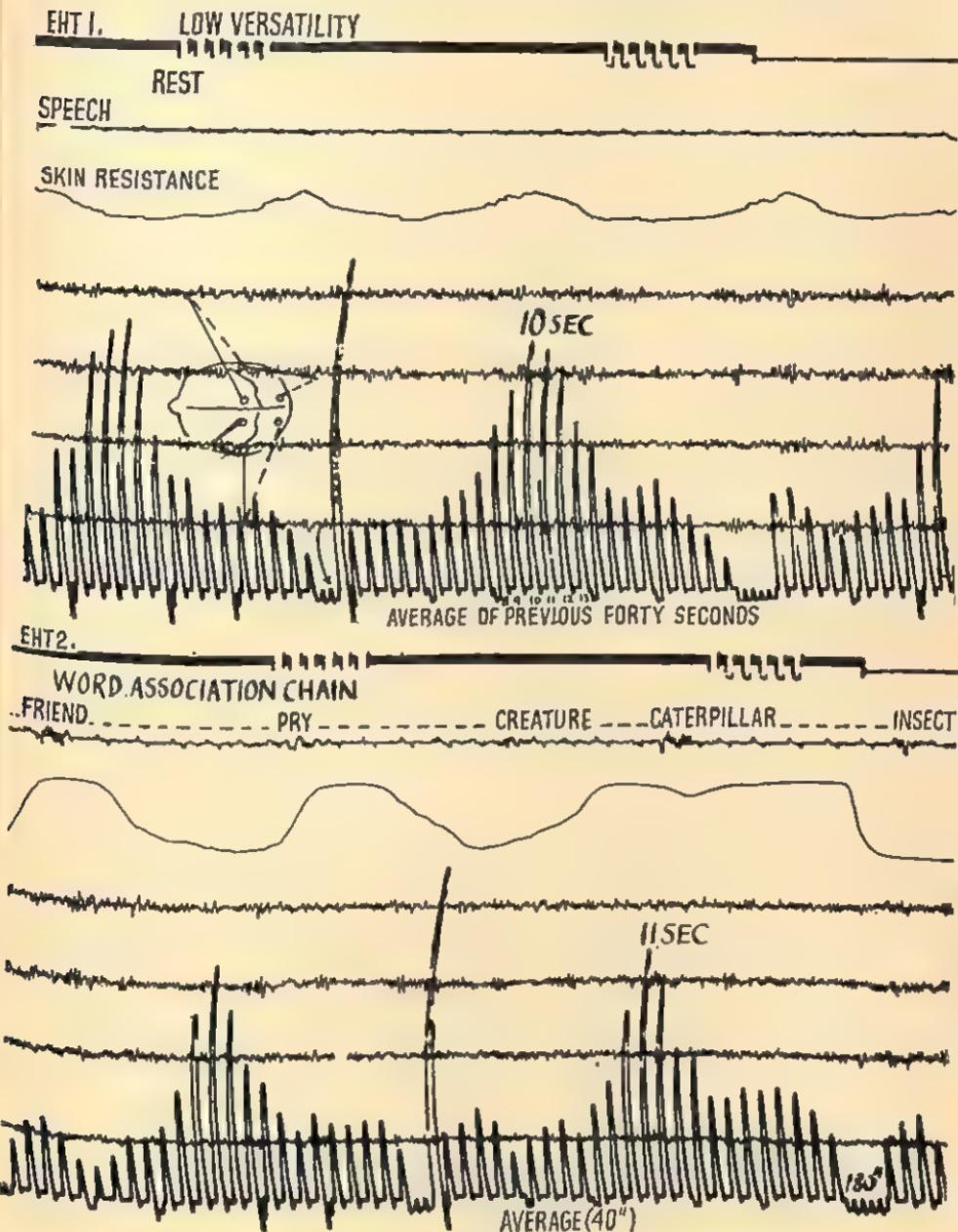


E.E.G. shows no dominant rhythm—her whole mind is active, working out this problem—then a little later she gave a great sigh, and grimaced, and later still, her muscles tensed, and the theta rhythm appeared, in that order. First came the breathing, then the grimace, the tensing of neck muscles, a drop in the skin resistance, acceleration of pulse, and, finally, the theta rhythm. At this point she made the remark, 'How far do I go?' She was worried about what she was supposed to do: the instructions given were quite vague; she had felt accurately the upright of the H and the cross-bar, but she hadn't found the other line. Then she got really annoyed that she hadn't got good instructions—she was a schoolgirl and used to being told exactly what to do.

Fig. 18B shows the further development of her theta activity. These are records taken continuously and you see that after the affective display you get a big peak which is the amount of activity at 5 c/s during than ten-second period. During that time the girl felt really frustrated, and didn't know what to do; as you see, her left hand is moving up and down, repetitively, constantly searching, and the right hand is not moving very much. The theta rhythm gradually, after a time, got smaller and smaller, and finally died away. Then there was another change in skin resistance and she returned to her normal state. Because of this frustration, she was quite incapable of solving this problem, one which every child that we have seen so far has been able to do. Her intelligence was quite high enough, but she got herself into a frustrating situation, and even at this age of sixteen developed an entirely infantile response of theta rhythm which was the essence of frustration for her, and which was the signal not to finish or solve the problem, but to get out of it, to regress to an emotional display. Then after the affective discharge, skin-resistance changes, the theta rhythm, the desire to get out of it all, you see her suppressing this display and making up her mind to be a good girl. She was a good schoolgirl from a good school, and we were nice people and she was not going to lose her temper on this silly problem. After she had got over this she solved other problems quite well. It is very characteristic during testing, as you know, in children of this age, that they may make several silly mistakes in the ordinary intelligence test, and then later on solve problems up to their age level perfectly well: this is just that kind of emotional instability that one sees at that age.

Fig. 19 has to do with another aspect of personality; the extent to which the alpha rhythms vary from time to time in the individual. This suggests the notion of *versatility* as a personality parameter which is at least measurable. Some brains keep the same tenor of activity hour after hour, even from minute to minute, from second to

FIG. 19
EXAMPLE OF TYPE OF RECORD MADE TO ASSESS 'VERSATILITY'



Above. The frequency analysis averaged over forty seconds during rest. There was little variation from epoch to epoch in the profile of the spectrum. *Below.* The conditions during the word-association chain test. The range of ideas was limited and each link was associated with a fall in skin resistance. The E.E.G. analysis shows a slight shift to the right in the modal peak but the profile as a whole is little altered.

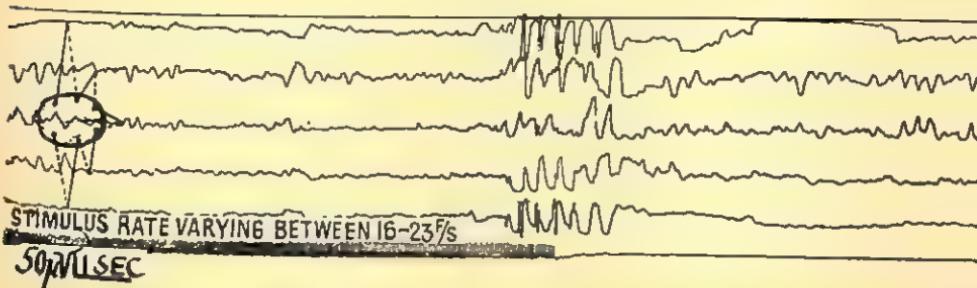
second. They have a small repertoire; little versatility. The other sort of brain may not be any better, in the sense of having a higher IQ, but is one which is constantly changing. This is something we can measure quantitatively and automatically and objectively. The record of Fig. 19 is an example. In it a girl, under a test situation, has her speech recorded at the top and her skin resistance below. The group of upright lines is the frequency analysis averaged over the previous forty seconds of activity. You see the profile of one spectrum is very much like the profile of all the others: this was a child with low versatility. The lower record shows the effect upon the same girl of the task of forming a word association chain beginning with the word egg. The record was taken toward the end of her association. At this point she had got to 'friend', 'pry', 'creature', 'caterpillar', 'insect'. The E.E.G. analysis shows a very similar profile to the resting one, except that the mode of the analysis, the highest point, is 11 c/s instead of 10 c/s. You may notice also that for each of the association changes there is a large drop in skin resistance. At the word 'caterpillar' she has managed to get out of the affective trap of 'friend, pry, creature'. You can see a slight tendency to produce theta activity which is suppressed, because she got on to the notion of caterpillar, and the chain was terminated just there.

Investigations of this sort on children have so far given fairly unequivocal results, showing that versatility can be measured from a very early age and, as far as we have seen, a child that is versatile in respect of one sort of rhythm will also be versatile in other respects, suggesting that this is a characteristic of the nervous system which can be measured and extrapolated with some safety. Versatility may have some connexion with the sort of things people like to do and particularly with the way people join up in groups. Two versatile people don't generally get on very well together; there is too much criss-crossing of ideas, the association is explosive and unstable. A versatile person and a non-versatile person get on very well together, because the versatile one will feed the other with ideas. The electro-physiology of these social groups seems to me one of the most fascinating things that one can study.

I have put before you these notions of ductility, of temper-keeping, and of versatility as personality factors. Now I am going to consider another factor which I suppose one could call *stability* or *balance*. This notion is related to the Pavlovian analysis of behaviour. Pavlov identified a character which he called balance in his animals and in his human subjects. This is important also in brain physiology, because one can show that the way in which an organism responds to violent stress varies all the way from a 'freezing reaction' of doing nothing at all, to a violent and sustained oscillation.

FIG. 20

WAVE-AND-SPIKE DISCHARGE EVOKED IN A YOUNG
DELINQUENT BY FLICKER AT ABOUT 20 FLASHES
PER SECOND



This discharge was accompanied by a transient lapse of awareness. This type of record is associated with considerable 'instability' and is commonest in epileptics

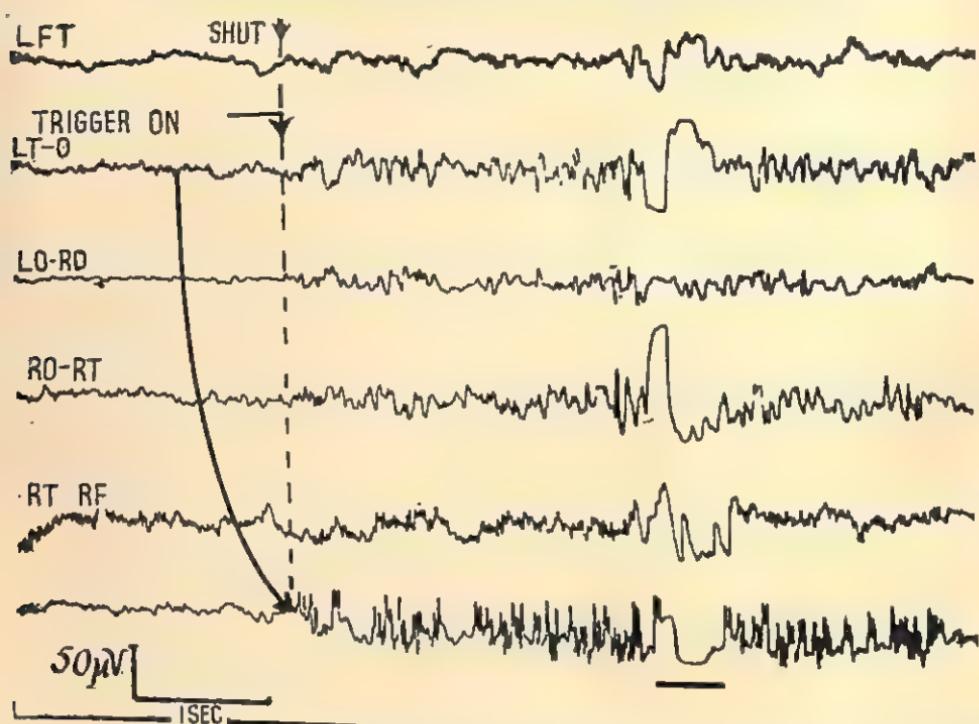
In Fig. 20 you see the record of a neurologically normal child who was a delinquent. He was at this time sixteen, and two years later he was convicted of a capital offence. He was one of the ductile type and he was also unstable. He had run away from home and required firm institutional control. This record is the response to a series of flickering light stimuli, which produce a gradually increasing fast discharge and then a large slow discharge. The whole brain is excited into an almost convulsive state by this simple flickering light. This is a response which one can elicit from about one in thirty of the normal population and is associated with the type of behaviour one sees in very young children. It is inappropriate, exaggerated behaviour. It is found in the sort of person who commits not a motiveless crime, but who instead of shouting at somebody or insulting them, knocks them down. If you get a combination of a lad with high versatility and normal E.E.G., and a boy like this with a high ductility and high instability, then almost always crime results, often murder.

In Fig. 21 we have a similar sort of picture. This is a record of a normal girl aged twenty, but here the stress has been made very difficult to bear for anybody, by making the stimulus itself depend upon the brain activity. The brain activity is made to operate a trigger circuit which provides the stimulus to reaffect the brain activity, and a feedback oscillatory system can be set up. Most brains are equipped to deal with this, which very likely occurs in the normal course of events. I suggest that it is one of the functions of the slow activity of the brain to deal with precisely this situation. Here we see the activity in the left occipital region arranged to provide the stimulus which is shown on the bottom trace. This bottom trace

FIG. 21

WAVE-AND-SPIKE PATTERN SIMILAR TO THAT IN FIG. 20

but less extensive and protracted, evoked in a normal adult by feedback flicker, the left occipital rhythms being made to generate a flash-stimulus.



The large slow wave (underlined) interrupts the reflexive mechanism, but the subject complained of feeling 'light-headed' whenever it occurred. The effect faded after about 15 repetitions.

copies the occipital one, and adds to it the stimulus, the sharp upward deflections indicating flashes of light which produce in this normal girl a violent discharge over the whole brain, leading up suddenly at the marked point to a series of very sharp discharges. Then these are cancelled in a large slow surge. The feedback chain is broken and this is what I mean by failure-to-safety. This may result in unconsciousness, and most people in this situation feel a bit 'swimmy'. This was the first time that this person had had this experience. If you repeat it, the effect no longer happens in a normal person. About a dozen experiences of this sort produces a gradual fading of both the fast and the slow response; in other words, the brain adapts itself in such a way that the retroactive networks are no longer established.

I believe failure-to-safety to be an important aspect of *petit-mal*. The reason why the wave and spike of *petit-mal* persists and recurs is that the brain does not reset its switches so as to avoid the retro-active arrangement. The mechanisms one sees at work here, I think, are those responsible in the brain for the four selective operations which are essential for the preliminary stages of learning. I think that these basic processes originate in the reticular system of the brain and are connected particularly with the estimation by the central nervous system of to what extent a set of signals are significantly associated. We are slowly collecting evidence for the view that the various operations necessary for learning develop at different rates in different children at different times; one may get, for example, a very mature and high selectivity in a child with no *constructivity* at all. The child has a very good discrimination and yet cannot retain in its mind a notion that two things are related. Conversely, a child may have a very highly developed constructive ability and yet be completely unselective.

PIAGET:

You said that there was a certain correspondence in electro-encephalography with our stages, but I must admit that I have not quite grasped where this correspondence lies and should like to have some details.

GREY WALTER:

It is very difficult to describe briefly what this correspondence is, and our own experiments are very far from complete. In stage I (of early infancy) wherein Mlle Inhelder distinguishes six different epochs, transitions from the second to third epoch, from the primary circular to the secondary circular reaction, seem to be related to the way in which the slow delta activity is interspersed with faster activity. The change from epoch three to four, from the secondary circular reaction stage to the co-ordination of patterns, where you get for the first time a real goal-seeking mechanism, seems to be associated with the time at which the electrical responses are not directly evoked sensory effects but appear in the temporal and frontal lobes as an abstraction of the stimulus. At this stage, instead of a response to flashes of light being merely in the visual area there will appear in the temporal or frontal lobes transient interwoven crystals of electrical energy which maintain for seconds or minutes the abstract form of the stimulus. The age at which this first happens is quite critical. Before this the signal reaches the sensory area, the child sees,

but it doesn't know the meaning of what it sees; then quite suddenly it does see the meaning. This is, of course, related to the ability to read and understand.

These experiments are rather tedious to do because they must be longitudinal; cross-sectional surveys are very misleading in this matter. If any of us are going to go on with this sort of work it's going to cost a great deal of money, because we have got to follow many children for years, examining them in enormous detail by methods that are quantitative, objective, and above all unobtrusive, because in making these experiments it is essential that one should not inject into the child the sort of thing one wants. It may be very difficult to work out a technique whereby you are not coupled too closely to your system, not yourself part of it, influencing it all the time.

MEAD:

Don't you need really to develop children that think that the whole of this measuring situation is part of life?

GREY WALTER:

Yes, of course, just like going to school.

TANNER:

I have a small point of criticism about your relating the curves of growth of keeping the temper from one set of data (Fig. 17), and electroencephalographic frequency from another (Fig. 15). I feel very dubious about relating two cross-sectional-type curves in that way. If the data are not taken on the same child followed longitudinally, the appearance of correlation may come about owing simply to general changes with age, though the correlation between the two variables in any given child may still be very small.

GREY WALTER:

That is a most reasonable criticism. In Figs. 15 and 17, I agree entirely that such relations can be quite meretricious. But we had already established experimentally a relation between theta activity and bad temper; the similarity between the two curves struck me as a particularly happy coincidence since the data were culled from quite independent sources, while our experiments with teasing children may have been spoiled by some sort of unintentional collusion.

MELIN:

I should like to know if Dr. Grey Walter can mention some sort of a pathological condition in a child which more permanently changes its electroencephalogram to a slow record and if this is accompanied by the related psychological manifestations.

Another question: we have seen how different emotions can influence the E.E.G., for example, how an angry child will show a changed type of activity. I have myself been doing some work in this field and found out how anxiety can influence the records and increase the amount of theta and also delta waves in the record. My question is: Can a child exposed to constant anxiety have its electroencephalogram permanently changed?

GREY WALTER:

I can't give an unequivocal answer: it is rather difficult to get children who have been through a traumatic psychological experience and arrange to observe them long enough and carefully enough to detect these changes and measure them; however, we have had the opportunity to see a few children as inpatients in whom just such changes have been observed. There was, for example, a boy of about twelve who had one of those peculiar psychometric documentations in which the results of various tests seem to be incompatible. The most striking feature about him was his inability to read. He was a very intelligent boy by non-verbal tests, with an IQ of about 130. His E.E.G. showed terrific profusion of alpha rhythms of all types. He had absolutely no visual imagination; but he had been taught to read visually. At the same time as these alpha rhythms he had a lot of theta activity and a lot of delta activity; he had an E.E.G. which would be more or less normal for a child of three. During the fortnight he was admitted, the E.E.G. record was completely transformed; his alpha rhythms became moderately responsive, theta and delta activity disappeared. His whole attitude to the problem of reading and the extraction of concepts from the mnemonic marks on paper was completely transformed by a process of non-visual tuition and separation from his family. Five years ago I should have said that records like this were unalterable. At one time I supported the statements of Lennox, in America, who said that the E.E.G. is an hereditary trait (LENNOX, GIBBS and GIBBS, 1945), but experiments we have done since that time have considerably modified my ideas. I have seen in E.E.G. records and in the psychological state of a child both regression as a result of injury followed by recovery, and also transformation from infantile behaviour and an infantile record, to a completely adult appearance as a result of quite mild psychotherapeutic treatment or the mere passage of time.

INHELDER :

I have been fascinated by Dr. Grey Walter's communication and should like to ask him two questions:

(1) Does the troubled stage you speak of occur before six years: that is to say, does it precede the first appearance of the alpha rhythm or does it come at about twelve years when the alpha rhythm becomes more regular and stable?

(2) Do you interpret the theta rhythm as indicative of stabilization of behaviour with attainment of control over self or as a phenomenon showing the adaptive reaction of a particularly emotive child? Dr. Monnier and I have examined a child of six years who showed a great deal of theta rhythm and we wondered what was the reason for this rhythm. Is it a question of a pronounced reaction to emotivity or of a stabilization of rhythm natural to this age?

GREY WALTER :

Was this spontaneous or deliberately evoked?

MONNIER :

It was spontaneous, but very much increased by hyperventilation. Why do you consider now the amount of theta rhythm as an expression of controlled temper rather than as the expression of affective bursts or increased emotionality as you described it previously?

GREY WALTER :

This is a change in my own point of view, based on what I have seen. The theta activity is associated with the practical need to control temper; in other words, in the child you speak of, you have a child who has a bad temper, and because of that, he has to control that temper, and it is the effort to control it which seems to be associated with theta activity. But there is something else underneath which we must study in great detail, and that is the possession of a bad temper, whether or not you can control it.

In answer to Mlle Inhelder's first question about the appearance of the alpha rhythms, it seems that rhythms of alpha frequency may appear well before six, in the back of the head; responsiveness to visual activity starts considerably before six in some children. You can sometimes find signs of alpha rhythms even in babies of a few months, but at the phase at which it appears and becomes responsive, there occurs usually a considerable vacillation in the child's behaviour. He will suddenly be able to read frightfully well, and then the next day he can't read at all. Or one day he will play with his toys and

bricks like a baby, and then quite suddenly he will go on to draw a picture.

BOWLBY:

This notion of ductility has puzzled me quite a bit since I first heard about it. What occurred to me was that the good child is not necessarily a normal child. Normal children are difficult, especially in certain situations, when they have their emotional and instinctual responses aroused in regard to special people, particularly parents. I wonder whether these ductile children haven't got some of their major social responses knocked out. One knows, of course, that that does happen to children in hospital. They are very difficult for a bit, then, after a few days or a week or two, they 'settle down' and are good. And everyone says, 'Isn't it nice, they are good'. They are being ductile, but they are also being very abnormal in the sense that, as people remark, 'They forget their mothers'. I was interested in the notion that the delta waves are related to a search for peace, because that is how one conceives of the separated young child: he gives up wanting his mother. To want and not get her is much too turbulent and awkward an affair so he 'forgets' her: he then becomes ductile—he has found a sort of peace.

LORENZ:

Dr. Sylvia Klimpfinger found in hospitalized children who were torn from their mothers, and who were exposed to a quick change of nurses, that they tried to form personal contacts with the new nurse, in a much weaker way the second time, in a still weaker way the third time, and then they gave it up. These children subsequently showed very different behaviour and could be divided into two types. One type was all over everybody, and the other type behaved like a chow dog that has lost its master—they became autistic. All of them showed a very great instability in their interests. They couldn't be kept at the same play for any appreciable period. They behaved then, especially the autistic ones, very much like delinquent children, with a general weakness of emotional reactions. These children who had their early reaction-to-mother knocked out were so similar to children who, either genetically or partially genetically, had a weakness of emotional and social reactions, that you could not tell them apart.

GREY WALTER:

The point against the explanation of ductility that Dr. Bowlby suggests is that statistically the children who had the most prominent

slow activity were those who had preserved most nearly intact a good attitude towards their mother: they wrote letters to her, they asked to see her, went back to stay with her during holidays, and so on, and during their leisure time they were the most socially active; they made models, they played games, and they were good company to their fellows; in fact their social orientation was most nearly normal. They were not those who rejected the mother-figure or had become hardened.

FREMONT-SMITH:

Was this information based on an average of a questionnaire answer?

GREY WALTER:

The isolation of this factor was based on the statistical results of an inventory of psychological and sociological inquiries.

FREMONT-SMITH:

Perhaps one would need to have a group of the typical ones examined more intensively. I think you might find that their relationship to their mothers was not as appeared in the statistical results.

GREY WALTER:

Yes, that might be so. These studies were made about three years ago; since then we have made a few more intensive studies and a few longitudinal studies, and as far as we can tell this still holds true: they are the children who have not rejected their mothers, who still keep in touch with them. One interesting thing we found was that the boys' attitude to their mothers was not necessarily associated with the mothers' attitude to the boys. The mother might be completely indifferent to the child, but the child would still be fond of the mother. That is not true of the father relationship. The relationship of the father to the child and the child to the father was reciprocal. If the father liked the boy, the boy liked the father.

BOWLBY:

I think that the extraordinary differences in the behaviour of these people in different social situations is most perplexing. In one situation they behave very satisfactorily and then, when they get into another situation, they go into quite a different gear. I am thinking of a well-known murderer who was head-boy of a delinquent school. He

was obviously an exceedingly psychopathic boy, but in that particular environment he could function as a social being; whereas in another situation where other stimuli reached him, such as sexual stimuli and love-object stimuli, his behaviour was quite different—he murdered his girl-friends.

GREY WALTER:

I wonder whether this ductility question isn't a matter of simplification of the response pattern. Perhaps the response of these children is not less normal, but less in quantity than that of a non-ductile child?

TANNER:

What is the relation between this measure of ductility and measures of suggestibility such as the Eysenck sway test?

GREY WALTER:

I don't think the psychologists used that particular test, but they did use a number of others and it looked as though certain aspects of suggestibility (the results, for example, of the Düss test) would relate significantly to ductility.

MEAD:

There are characters we call in jargon 'working idiots' in New Guinea and Melanesia. The only idiotic thing about them is that they work; they are people who will do things just because you ask them to. They are not economically forced to do this. They are not stupid, and they are quite well recognized through quite a large cultural range. This is against Dr. Bowlby's explanation as compared to a genetic one, because they occur in societies of such extraordinarily different mother-child patterning. People regard them as a resource. My suspicion would be, from the distribution in New Guinea, that there is a genetical element.

FREMONT-SMITH:

They might not be really parallel to ductile children.

CAROTHERS:

I have three questions. The first is, that I understand that the alpha rhythm has its main centre, as far as the cortex is concerned, in the occipital area: I was wondering if the theta was similarly associated

with any particular part of the brain? The second question is: is there any relation between the type of rhythm and whether the subject tends to use auditory or visual imagery? The third question is, in view of the difficulty of comparing unsophisticated people with sophisticated, and the difficulty of making the African calm and at ease in a test situation which is very frightening for him, would there be any advantage in comparing these people in sleep? Does sleep vary during life in any constant pattern as one grows older?

GREY WALTER:

The distribution and geometry of the alpha rhythms are extremely variable, but certainly they are oriented longitudinally over the back of the head rather than laterally at the side. There is a relationship between the distribution of the alpha rhythms (which are always plural) and the way the information coming into the brain is distributed and relayed around the surface of it. Now compare that to the theta geometry. There are many sorts of rhythms in the theta, but the most prominent one is at right angles to the alpha field; the alpha rhythm is fore-and-aft at the back, the theta rhythm athwart the head at the side, mainly in the temporal and parietal regions. In certain parts of the brain these two interlace. At a certain age of the child you may have alpha and theta activity going on at the same time. The part of the brain where we see sensory signals first abstracted, or given meaning, is the part where this interlacing is usually most intricate, where the texture is most elaborate. You get a sort of tartan of these spontaneous rhythms, and it is where this tartan is first formed that you get the most highly abstracted and elaborated and preserved pattern of incoming signals. In other words, it looks—on a completely superficial and possibly mistaken view—as though the pattern-seeking and pleasure-seeking mechanisms were interlaced in some way, like the threads in a sewing machine.

In relation to the question of examining Africans or any people with a different background, this is obviously most important, because, in order to compare two populations and get any idea at all of what an African's brain is like, one has to make an extremely elaborate analysis of the whole situation, to see how he looks at it. In other words, you must be an African or train an African to study his own people; it seems to me that is the only way we can do it. One simple example of that: most people in laboratories wear white coats; for most children a white coat is a danger-sign. The result is that if a child in our laboratory is examined by somebody in a white coat you may get one sort of a record. If he is examined by someone in a green coat you may get an entirely different record.

To go on with my answer to Dr. Carothers, I don't know very much about rhythm and imagery in Africans. In an English population, about 70 per cent of the population have a mixed imagery type: they can turn on any type of imagery they need, more or less, within reason; about 15 per cent can only use visual imagery, they tend to be obsessed by visual images, and can't do much else; and about 15 per cent cannot produce a visual image if you pay them.

CAROTHERS:

The other question was about sleep.

GREY WALTER:

Sophisticated and unsophisticated people studied in sleep. The sleep pattern as it is described in textbooks is not much like the real sleep patterns, which are much less rigid. There is enormous personal variation in pattern, but there are certain features that do occur very commonly. Always at some stage you get slow rhythms, always you get spindles. The first stage is usually a theta stage, which is a 'floating' stage when you feel your body disappears and you are floating—that is usually the time when the afferents are cut off from the body—but the alterations of these phases, and the amount of time occupied by each one during diurnal or nocturnal sleep, is an enormously personal factor. It seems to be set at a very early age and to maintain itself throughout life. You can predict to a certain extent from the examination of a child's sleep record the sort of sleep he will have when he grows up—apart from emotional disturbances and so on; and even then you can predict, to a certain extent, from this very early infantile characteristic what effect such disturbances will have on a person's sleep. We don't know whether there is any ethnic variation in these characters, or if the situation is different in peoples who can sleep very easily. Nor do we know whether sleep patterns are susceptible to conditioning. We know from Pavlov that sleep can be conditioned; it is one of the standard effects of delayed and trace reflexes. But even when tried out in dogs it is found to be characteristic of the individual dog, and to be one of Pavlov's typological criteria.

CAROTHERS:

But is there no close parallel between the degree of development during waking life of the individual and the rhythms in sleep during the same period of life?

GREY WALTER:

You mean as a general rule throughout? No, you may get the sleep variations and the adult variations behaving quite differently. I don't say there aren't some correspondences, but superficially considered the two things are different mechanisms. That is probably simply because we haven't used the right sort of stimuli in waking life.

KRAPF:

To what extent do you believe that the different electroencephalographic patterns could be related to Rorschach studies? Just lately, in Yale, they have developed differential patterns of the Rorschach in different ages of childhood (AMES, 1952) and it struck me there might be a parallel, for instance, between form and percent alpha or maybe the Erlebnistypus and the delta pattern and so on. I feel that this might give us an interesting slant on our problem.

GREY WALTER:

We have data on a number of cases. There was a boy who had a number of fits whose Rorschach was grossly abnormal when first seen. His E.E.G. also was highly pathological, immature, and retarded, and showed some epileptic features as well. He was then given anti-convulsant drugs, which controlled the seizures quite well. Three months later he was seen, at the age of about eleven years and three months. The Rorschach was repeated, which, of course, was a doubtful thing to do, and there was complete transformation to normal. His E.E.G. also was then normal. We thought this might be due to the drugs, so we took him in as an in-patient, withdrew his drugs for two weeks, and though he did have one seizure, his whole personality remained quite stable. So in that case within the space of three months we were able to see a complete transformation from an immature, quite pathological, child with an abnormal Rorschach, abnormal behaviour and abnormal E.E.G., to an integrated, almost adult person.

RÉMOND:

I should like to say a word on the question of choosing longitudinal or cross-sectional series. It is true that statistical population studies found in the literature are mainly cross-sectional studies. Today it is considered that longitudinal studies, even when much more restricted, are of much greater interest. Personally I consider that certain cross-sectional studies would continue to offer great advantages where very homogeneous groups are used.

At present I have the opportunity of examining a group of 200 persons. They are cadet pilots of twenty to twenty-four years who have been medically examined and have the physical and intellectual aptitudes qualifying them to become pilots. They form, then, a very homogeneous group. We insisted on getting this group because the Air Ministry in Paris asked us to define the criteria of E.E.G. normality for acceptance or rejection of cadet pilots. For a long time we refused to take such a responsibility, but during a recent experiment, when we were studying the effect of stimulation on the electroencephalogram in an attempt to find proofs for the existence of an epilepsy of which there was little evidence, we noticed that the threshold above which these cadet pilots responded to stimulation seemed much lower than that which had seemed to us to be normal up to then. The study of about fifty individuals taken at random and from different age-groups had in fact given us a vague statistical idea of what was the normal threshold.

These very low thresholds of stimulation seemed very curious and we wondered why these individuals were, in relation to our previous work, pre-epileptics. Our psychologist friends, having considered the problem, then suggested that our subjects had wanted to become fighter pilots because they had this particular characteristic which we considered as being pre-epileptic.

Recently when we began to examine a new group of 200 cadet pilots we were surprised to discover again, even from the first ten, that the average electroencephalogram and the average reactions to various sensory stimulations were very different from what we had thought up to then to be the normal aspect for that age. We found with this group an abundance of slow waves in the parieto-occipital regions and a notable quantity of rapid rhythms in the anterior regions. These rather special characteristics make us say: take care, this is perhaps an abnormal individual. If we took his epileptogenic threshold we should no doubt find it very low; if we examine him thoroughly we may find traces of a past pathological attack, although the history has allowed us to think there has been none.

Given the very homogeneous constitution of this group, which is a selected group, the common characteristics we shall find among these persons will be far more valuable and significant to us than if we had carried out this study on a cross-section of the population of the same age, but taken at random.

If I might add another conclusion I would say that this cross-sectional study will cost less than a longitudinal study on say 10 per cent of the same subjects. This study will become more valuable when it is compared with other studies of very homogeneous groups such as we have already begun to consider. Thus we are going to be

sent a group of French railway employees chosen to be engine drivers. Naturally, we always choose individuals without, or with very little, neurological history, and I think that we can thus obtain some guiding points for discussions, I will not say on the normality of an individual but on the aggregate of his special characteristics.

ZAZZO:

I agree with Dr. Rémond, but I think two things should be specified: on the one hand, the necessity of constituting a valid sample, and on the other, for a study in genesis, the need to proceed longitudinally.

I do not think Dr. Rémond's arguments give a full reply to the problem of choice between transverse cross-sections and the longitudinal method, since the example he has furnished concerns adults and the problem for him is to choose a significant group among these adults. I therefore think that the question remains open and that although in some cases the cross-sectional method may suffice, in others it may be very dangerous. Thus, if we study girls of thirteen the cross-sectional method is meaningless, because some will be pubescent and others not. In this case the cross-sectional method cannot be used. Therefore I think it is necessary to distinguish between the two questions, that of the constitution of the sample and that of the choice between the longitudinal method and the cross-section method. The question of sampling always comes in but it can be differently solved according to the method chosen.

RÉMOND:

I should nevertheless like to say that the first series in which we were interested comes within the bounds of this group's study because the growth of the subjects is not finished. I think the end of growth comes at about twenty-five years. However it may be from other points of view, as regards electroencephalography this group was shown to be relatively infantile. We do not think that these characteristics are any more evident among younger children. Moreover, this group will help us define one of the particular aspects of the end of growth or development.

SIXTH DISCUSSION

Stages of Psychological Development of the Child

ZAZZO:

I intend to comment briefly on the report drafted by Professor Wallon, thus coming back to the stages of early childhood, especially to the first year. After that I will go over the stages in the period of schooling; then I will take two sectors of behaviour, the evolution of graphic ability and the evolution of language, and refer very simply to the main stages, as they have been defined by a large number of authors. Then I will conclude with some considerations on method which seem to me common to psychology and all the branches of biology.

I have been asked to prepare a report on the work of the Wallon School and also on some of the work of Gesell, whose pupil I was. It seems to me already significant that Mademoiselle Inhelder and myself should have been asked to give an account of certain psychologies—I am purposely using the term in the plural: the psychologies of Wallon, Piaget, and Gesell. It seems that there is not a science of psychology, but a group of systems. We can, however, ascertain by reviewing psychological literature that there are extensive data on which to base an objective science. It is, therefore, not lack of established facts which leads us to speak of psychologies in the plural. Because of the nature of psychological facts, and perhaps certain biological facts too, organization and interpretation are constantly necessary and these established facts, if they are to be used, require a system in the present state of our knowledge.

Motor Development in Early Childhood (according to Professor HENRI WALLON, 1925, 1946, 1947, 1949)

There are two ways of presenting the motor development of the child. The first is to give a description of the child's reactions in the chronological order of their appearance and in relation to their physiological context as well as to the circumstances of the moment.

This is the basic method; but it calls for much detail and can lead to confusion owing to possible anticipations or frequent overlapping between manifestations differing in significance. The second is the functional method which groups these manifestations according to their nature. It is synthetic and more interpretative, but also more distinct and explicit. It is the method adopted here.

(1) *Automatisms of Posture*

Co-ordinated reactions of the head, the limbs, and the trunk in response to certain exciting causes can be observed from the foetal period onwards. Such, for example, are the labyrinthine and cervical reflexes described by Magnus and de Kleyn. They are easily demonstrated in premature infants and also, under favourable conditions, in those born at term, by rapid displacement of the body upwards, downwards or horizontally, or by change in the orientation of the head relative to the trunk. They soon disappear. It would seem, nevertheless, that this series is connected with the reactions consisting in adjusting differently the segments of the body to each other and to exterior supports. To begin with there is the reaction acquired during the first few weeks: the active lifting of the head and its lateral displacement by means of the muscles of the nape of the neck. After that comes a whole series of readjustments arising from the child's contact with the ground in attempts to sit down, to stay on all fours, to stand up and finally walk. The difference between these reactions and the reflexes of Magnus and de Kleyn is that instead of being purely passive they are the result of active ventures into the outer world. It is the same difference as exists between intra-uterine life and life in free space. The automatic and unreasoning character of the first reflexes can be found again in the sudden emergencies of physical danger; they are components of actions with a fixed object.

(2) *Tonic Activity, Attitudes, and Means of Expression*

It is difficult to distinguish at birth between the spasms arising from tonic activity and actual movements. Cries, and the accompanying gestures—particularly stiffening of the trunk, opisthotonus, and sudden tensing and relaxing in the arms—appear to be definitely more spasmotic. The motor discharges in the lower limbs have the more dynamic character of pedalling. Perhaps there is a period where tonic and clonic activity are still poorly differentiated; this could no doubt be discovered through electrical examination of the muscular apparatus. It is, however, tonic activity, both of the visceral muscles (respiratory, laryngeal, and gastro-intestinal) and of the skeletal muscles, that corresponds to the effects noted. Its proper field is that

of attitudes and expression; in other words, that which is of greatest use to the child, since only by these means can he obtain and solicit from his surroundings the help which is essential. The visible effects of his needs, his discomforts, and his sufferings soon link up, by means of conditioned-reflex mechanisms, with the tutelary effects which they have caused, and eventually become a means of arousing them. Towards six months the expressive manifestations of the child are so finely graded that all the major varieties of emotion can be distinguished. Vocal intonations precede language. The voice, the medium of speech, is a tonic activity. The attitudes which result essentially from tonic activity establish contact between the individual and the corresponding situation, underline the meaning of this situation and contain already its image.

(3) *Circular Activity*

There comes a period where the resultant effects of gestures tend to make them more specific, for example, the hand which enters the visual field and obscures the view, or the sound produced by certain muscular contractions of the vocal organs. This association must mark the moment when the fields of kinaesthetic, visual and auditory etc., imagery fuse together. Then what Thorndike calls the law of effect, or what Baldwin had described as circular reaction, comes into play. It is an important factor in the psychomotor development of the child. The effect obtained excites the movements which have produced it, and inversely. All kinds of learning processes follow (vocal noises and language phonemes, autopalpation and recognition of body activity, etc.). This period belongs to the second half of the first year.

(4) *Movements Towards External Objectives*

The first meetings with external objects seem to cause only reflexes without relation to the objects themselves. For example, the hand which closes over whatever touches the palm does not show prehension but rather a clinging contraction which is closer to the actions of holding on or climbing than those of picking, seizing, or grasping. The period when the child becomes capable of reacting to an object as such appears towards eight or ten months. It is the period of 'near space' (W. Stern), a period of exteroceptive and exteroffective exploration, of catabolic elaborations rather than anabolic, such as those which could be called autoplastic and which are directed towards the construction of the individual, not only of his organic basis but also of everything which can give him shape and form.

Motor activity can develop in two different orientations which are, moreover, interdependent: one, whose source lies in the sphere of

attitudes, that is to say in tonic activity, but whose later developments can use all the other forms of motor activity or mental activity (imitation, for example); the other could be said to be extraverted, seeking its effects in the outer world, but requiring the first in order to become precise and consolidated. A large number of activities can be considered alternately from one or the other point of view; for example, motor habits, according to whether they are related to their object or to their learning.

(5) Motor Stages and Syndromes

The successive stages of motor development, which moreover overlap each other, can then be distinguished as the *affective-motor* stage, the *sensori-motor* stage, and the *objective-motor* stage, which could also be called the projective stage, since the perception of an object calls for a total projection of activity, the perceptive or conceptual settings remaining indistinct or inert. In every motor act, however, intervene neurological components, which can be more or less retarded or deficient, one or the other. As a result certain functional deficiencies or fragilities occur, which lead to different syndromes or motor types, each in connexion with the corresponding psychic dispositions.

The regulation of tonus can be affected in different ways. Some children in the larval state, and depending on the occasion, show the stiffness which is observed in Parkinson's disease. In others, of a quite different psychic type, the hypertony is shown by contractions of the face or the body such as occur in certain pallidal lesions. They are mobile and can eventually give way to generalized muscular relaxation.

There also exist cases which closely resemble cerebellar asynergy. Another syndrome comprises muscular instability accompanied by tremblings and very slight irregular displacements of the head, shoulders, trunk, and limbs, which if enlarged would give a picture of chorea and which I have called for that reason subchoreic instability.

Finally, there are various syndromes which appear to refer directly to the functions of the cortex. It seems possible to localize two of them fairly accurately. One of them is basic to apraxia. It concerns the global comprehension of the motor act and its carrying out according to well arranged phases. The other affects that field of psychic activity which is closely related to attitude and particularly with those reactions which can be aroused in us by the presence of another person and which I have called reactions of bearing. The syndrome would be one of prefrontal inadequacy.

This brief outline has, of course, not taken into account the many details whose description here would lead to useful conclusions or

confirmations. It is only intended to give the main lines of the functional plan with which psychomotor activity complies.

I will not dwell on Professor Wallon's system. I shall simply try to show where the PIAGET (1951)-WALLON (1947) controversy lies as regards these ideas.

Personally I do not believe that the opposition between Piaget and Wallon, which some have wished to establish and maintain, exists. I have the impression that they have each put themselves in a different perspective and have considered things according to their own temperaments. Moreover they illustrate very well how, starting from the same established facts, divergent interpretations can be evolved, which are not, however, necessarily in opposition.

The two aspects which seem to me different are the following:

Firstly, with PIAGET (1936, 1947), as far as the infantile stage is concerned, the interest is focused mainly on the genesis of sensorimotor intelligence. For him the essential is to get a grasp of the very first elements in this genesis, even before the appearance of sensorimotor intelligence. He searches for the most fugitive signs in the very first weeks of life.

Wallon, on the other hand, is preoccupied with something entirely different. He studies by his own dialectical method how, starting from the same organic sources, very different types of behaviour separate out. Thus, deriving from the motor activity he observes between the ages of three and six months, he finds motricity orienting in two different directions. The first is an aspect of motricity which is in a sense made of tonic material and constitutes a starting-point for affectivity and for syncretic affective sociability. The second orientation is the sensori-motor sequences, those circular reactions which will be the starting-point for the idea of object and for representation. Wallon attempts to show how these two aspects of human behaviour, starting from the same organic sources, finally determine each other.

The second difference, or at least apparent difference, between the two systems concerns just this idea of syncretic sociability. This is where PIAGET (1926) and WALLON (1951) have been most strongly placed in opposition to each other by those who in my opinion have schematized both.

Generally Piaget is schematized thus: the child starts from a sort of autism—moreover a relationship is established between Piaget's autism and psychoanalytic autism—and gradually the child becomes a social being. So, starting from an egocentric state—and even, going farther back in childhood, one might speak of a sort of solipsism—from which the child at about six years socializes himself until at about twelve years he achieves the feeling and idea of reciprocity.

According to Wallon there can be no question of autism in early childhood—unless, of course, the term is redefined. Wallon insists on the extreme sociability of the small child and says that probably at about six months the child is the most sociable of beings, having that syncretic sociability which he has defined. Then evolution will not be from a state of autism, of withdrawal into oneself—which by the way Piaget never said—to a social state: progress will start from a state of non-differentiation, a state of communion with the mother, and will gradually reach a definition of interdependence between self and others. At the same time as this evolution is taking place on the affective plane, sociability is taking on another aspect: it is becoming more intellectual, while continuing to retain this affective, emotive basis, which can be found still even in the adult.

There is, therefore, no opposition between these two authors, but they treat different functional levels of sociability. Personally I regret that the words 'autism' and 'egocentrism', used by Piaget with all sorts of precautions, should have given rise among careless readers to false interpretations.

Main Stages of Development from Three to Eighteen Years

I should now like to pass on to the second part of my communication, that is to the evolution of the child between three and eighteen years. Rather than giving a complete and dogmatic exposition of this problem I will refer to certain stages which we might discuss.

I shall now abandon the frame of reference used by Wallon the neuropsychiatrist and neuropsychologist and use one which contains nothing scientific: scholastic organization as it exists in most of our countries. Whatever your opinion on the value of scholastic programmes and pedagogic methods, you will no doubt admit that the school must take into account, at least approximately, the child's possibilities, and that the main scholastic stages schematically interpret a sort of implicit psychology. Moreover, historically the question of mental age originated from the question of school age. Age differentiation became useful only when compulsory education was instituted in most of our countries. The identification of the stages and forms of intelligence became more precise with the diversification of studies as formulated, for example, in the British Education Act of 1944 and the French 'Plan de Réforme' of 1947, known by the names of its chief promoters, Langevin and Wallon.

To come back to the scholastic stages, let us see how the psychologists have defined their significance and their limits.

Between three and six years there is a pre-school stage in almost every country. Attendance is optional, and there is not so much instruction as education—at least that is so in theory.

From six to twelve years comes the stage of primary schooling, which consists in the acquisition of the basic intellectual automatisms reading, writing, and counting.

From twelve to about eighteen comes the stage of so-called secondary schooling which culminates in the matriculation (*baccalauréat*) type of examination.

Beyond eighteen years comes the post-school educational stage or university studies.

It is interesting to note that these ages, three, six, twelve, eighteen years are the same in most countries with a variation of about one year. It is also interesting to consider that reformers, when demanding an extension of schooling beyond twelve years, no doubt take into account reasons of a social nature. They want to democratize the school, open the school to the whole of the population and give equal opportunities to all children. But according to the most recent demands for reform, we see that another argument is invoked: in actual fact the intellectual evolution of the child is not finished at twelve years, and to achieve a harmonious development of the individual instruction should be prolonged until fourteen or sixteen years, perhaps even to eighteen years. It is instructive in that respect to read the 'whereas' clauses of the British and French plans for reform.

School practice has not only prolonged the limit of studies but has also determined the sub-divisions within the main primary and secondary stages. By and large a sub-division is marked at nine years, when the mechanisms of fluent reading and writing have already been acquired (end of elementary course according to French terminology). Another sub-division is made at about fifteen to sixteen years, as shown, for example, in the two stages of American secondary education (junior high school and senior high school) and the two stages of the French grammar school (orientation stage and determination stage).

It seems then that the school in our different countries has marked the following stages: three, six, nine, twelve, fifteen, and eighteen years. It is to be noted that there is some kind of concordance of psychological studies with these different stages.

As for three years, for a long time psychologists have marked this as a period of crisis and of socialization accompanied, moreover, by an attitude of opposition. The child experiences a certain confusion and uneasiness and yet new possibilities of the beginning of a collective life are appearing. The nursery school or kindergarten answers the needs of this age.

At six years an emotional stability is seen to appear in the child. The child enters the phase called by psychoanalysts the latency

period which, according to other authors, is a period of objective interests. One fact we can definitely establish is that the child becomes capable of fixing his attention and concentrating for fifteen or twenty minutes on exercises he would have found very boring previously. Dr. Grey Walter mentioned the same phenomenon in his communication: at about this age the E.E.G. takes on a fairly characteristic form. The alpha waves are dominant. We have been able to ascertain by using a very old test, the crossing-out test, that the child of five years is quite incapable of this effort. For about ten years we have been using a rather special technique which consists in giving two degrees of difficulty in this crossing-out test: in the first, one sign has to be crossed out; in the second, two signs have to be crossed out. Thus the reactions of a child faced with two degrees of difficulty can be compared. At five years the child is incapable of carrying out this test. He abandons it very quickly. At six years the task becomes possible; 60 to 70 per cent of the children are capable of performing it. At seven years the adult formula is already established. At six years the formula for behaviour is very characteristic: the child's activity, although well adapted when he has to discriminate a single sign, tends to become disorganized when there are two signs.

From the age of seven years the adult formulation is reached, though naturally with a difference in speed. I think that this shows an essential phenomenon: the possibility of concentrating on tasks which do not involve the immediate interest of action. The child is capable of sustaining his interest in order to act on an intellectual plane.

At about twelve years a very remarkable fact is noted: the most diverse psychological methods are unanimous in describing this age as a sort of culmination. Beyond twelve to thirteen years the tests are hardly discriminatory, or rather they are no longer discriminatory as tests of development. It would seem that individual differentiations are then more apparent than differentiations due to age. We all know of the work carried out during the First World War where it appeared that the mental age of the recruit, the ordinary soldier, was twelve years. This does not mean that our armies were composed of mental defectives; it means that the age of twelve marks the culmination of a certain type of development. The same conclusions are reached in Piaget's work, which shows that the principles of conservation have been acquired on all levels by this age.

However, we certainly have the impression that evolution continues beyond twelve years. How does it continue? It is a fascinating problem from the physiological, neurophysiological, psychological, and social points of view. Unfortunately our documentation on this subject is much less sound than that on the child of zero to twelve years.

Much has been written on adolescence but in my opinion very little of it is valid in the field of psychology. One notes that the adolescent as described in psychological works may be the adolescent of a certain epoch and particularly of a certain social milieu, that is to say the milieu of school children and grammar-school pupils. Does this very romantic picture of the adolescent, with this profound crisis, this personal upheaval, correspond to reality? I do not believe so. Of course this type of adolescent exists; I have certainly met it in the grammar schools.

We have carried out certain comparative studies on adolescents in different environments, some already working in factories, and others continuing their studies. It is most surprising to discover that among those who have already entered a social life a hostile attitude towards the family is very seldom met, whereas this attitude is at its maximum among grammar-school pupils of the same age with the same material conditions, that is to say, having as much pocket money as the young worker. It is not, therefore, the fact of having money which explains the attitude of these young people, it is the fact of earning it or receiving it. Moreover, a much earlier autonomy is found in the young worker than in the young grammar-school pupil. Obviously this is not sufficient to give a valid differential picture because, even if we find here that the worker has superiority over the grammar-school pupil, on the other hand, the grammar-school pupil has the superiority in that he becomes aware of cultural realities.

Our knowledge of the adolescent is still very imperfect. Perhaps what we understand best is what is revealed on the intellectual plane by the possibility, for a boy or girl of fifteen to sixteen, of attaining on the practical or theoretical level a hypothetico-deductive attitude. What does this signify? I will make a very shaky hypothesis: at about twelve to thirteen years all the fundamental mechanisms have been acquired. Beyond that the cultural element comes into play much more than the developmental elements, although evolution continues on a physiological level, but the mechanisms already acquired at twelve to thirteen years still need to be made flexible, through exercise. In this connexion SIMON (1939) said that it was not sufficient to have intelligence, it was necessary to know how to use it. Perhaps it is just after twelve years that this intelligence begins to be used in different directions. I mean by that that psychologists are perhaps wrong in looking only for the limit of intelligence. The age of twelve to thirteen years marks perhaps a new departure. Intelligence is nothing if it is not creative and intelligence can only be effective if it goes back to certain affective sources. Now it is perhaps during this period from fourteen to eighteen years that the human being,

coming into contact with new social, human, and affective realities, manages to give a deep, concrete sense to all the perceptive-intellectual mechanisms which he has acquired during the scholastic period.

In short, I wished to underline our lack of certainty about this period of adolescence, on which much has been written. This study has been perverted because not enough account has been taken of the cultural and social dimensions which give direction to adolescence and without which this period cannot be defined. I think that beyond twelve years cultural factors are of such importance that it is necessary to proceed by a differential means in order to reach significant results.

Evolution of Graphic Ability and Language in the Child

I will now pass on to a statement on two series of behaviour: evolution of graphic ability and evolution of language. For these examples we shall return to early childhood. I apologize again for the schematic nature of this sequence but it is only intended to serve as a basis for discussion.

On the subject of the evolution of graphic ability (AJURIAGUERRA *et al.*, 1949) I should like to stress the fact that it seems to me to be of capital importance and that it starts at about the age of eighteen months. The child then becomes capable of copying a vertical or horizontal line, but he can only copy the line right next to the model line. We frequently observe this phenomenon of 'sticking close' in the child's first graphic images. This tendency seems to last up to two or two and a half years.

At three years, the child becomes capable of copying a few capital letters but he copies as he draws; the cursive movement of writing appears about six months later. The child very early has a different attitude to drawing from writing.

At four years he is capable of copying words but he puts them anywhere on the paper. At the beginning he writes at the bottom of the page and at about six years, under the influence of instruction, he begins to write at the top on the left.

At five years he can copy phrases and becomes capable of writing a few words spontaneously, but with many inversions in letter order.

At six years we have observed a very interesting phenomenon: the appearance of the human profile turned towards the left. This phenomenon has given rise to quite a number of interpretations. In any case on the intellectual plane this shows fairly considerable progress.

Up to five to six years graphic ability is determined by motor ability. The dominant hand develops less rapidly than the other with a retardation of one stage. I should like to recall here the law propounded by GESELL (1945), the law of reciprocal overlapping, according to which the definitive formula on the motor plane is not

established gradually in the child but by means of a play of alternation. These alternations, according to Gesell, are the sign of a deeper alternation due to myelinization.

Beyond six years the intellectual and affective factors are decisively added to the motor factors in the evolution of writing and drawing. A number of authors have studied the psychology of drawing, notably LUQUET (1913), who distinguished different stages as, for example, the passage from what he calls intellectual realism—a stage where the child draws with an effect of transparency and of skew—to visual realism, which is attained at about nine or ten years.

At nine years orientation of graphism towards the left is noted (*sinistrogyration*) not only in writing, but also in drawing. This originates as a motor effect and attains a sensory form.

In this connexion I carried out the following experiment (ZAZZO, 1950). A child is given some very diverse figures and is asked to discover profiles in these pictures, which are shown very rapidly. At seven to eight years there is no difference, he finds as many on one side as on the other. At nine years almost all children see only the profiles which are turned to the left. We wanted to confirm this law in adults and we found the same phenomenon.

In short this perceptive organization, this sensory evaluation, appears at nine years, and from nine years on the same percentage is found as among adults. I make one reservation, but at the moment I cannot give any definite interpretation of it: during adolescence, between twelve and fifteen years, an extraordinary reversal of these proportions is observed. I do not, however, think that there is a perceptive disorganization at this age. During this period the right profile takes precedence. Is this a sign of a tendency to opposition, a disruption on the perceptive plane due to emotive factors? I cannot say. I pass this question over to the competence of those who are more directly concerned with the problem of affectivity.

I will carry on then with the history of graphism. Among the various observations that have been made I find stages and ages which appear to me particularly significant. With the appearance of organized graphism, the first stage which I consider to be important is the disappearance of this graphic confusion which expresses on the graphic-motor plane one of the fundamental characteristics of childhood—non-differentiation. This stage comes at about two and a half years.

Still another important stage is that at six and a half years when the child is capable of well-organized drawing and attempts to write.

Another important age is that of nine to ten years. At this age, when the profile experience occurs and where it is no longer the hand which is responsible but the eye that guides the hand, the child is

found capable of dissociating himself from motor conditions and dominating them. There is an organization of a perceptive field, a sort of perceptive Gestalt, whose genesis we have already seen; there were first neuromotor determinations, then towards nine years demands of a sensory nature which dominated motivity. It is at this age that a child's drawing attains conformity to appearance. It is also at this age that he attempts to read and write fluently. I think that the convergence of these facts is highly significant.

Beyond ten years the story continues but the cultural factors markedly dominate developmental ones. It is no doubt necessary that intelligence should continue to evolve for the child to be capable of giving chromatic relief to perspective, but it is obvious that education is a most important factor and that a person who has received no instruction cannot produce perspective even if psychologically and physiologically he is capable of doing so.

It is interesting to note that all the comparisons that have been made on children of different cultures show the same evolution up to ten years. One of our colleagues, PRUDHOMMEAU (1951) applied drawing tests to different ethnic groups, Eskimos and others. Exactly the same stages of evolution were found. No difference has been found between French children and children of other nationalities that we have been able to examine.

I will now pass on to the problem of language. It would be very interesting to define the common points by comparison of these different lines of behaviour.

In the first two months, there is wailing and emotive crying.

From three months onwards we get babbling and warbling, and certain utterances gradually take on an expressive character. According to PICHON (1947) this is pre-verbalism which has, moreover, been confirmed by most of the English and German authors and has been fixed at the age of about six months.

Then comes the stage which certain authors have called the stage of pure comprehension: a child understands but cannot yet express himself.

The first word appears towards the end of the first year, but it is very difficult to fix a date for the first significant word. There is much discordance among authors on this point and by saying that the ages are considered to vary between nine and fourteen months I will not have added anything very interesting to the discussion.

Are there any natural words which are found in all cultural milieux? The word 'Mama' has been suggested and the negation 'no', which marks a movement of refusal of food. One thing is certain, that is that at nine months (possibly a little before) a child is capable of imitating certain syllables. The child has the lip movements which are

structured into sounds or phonemes. Is the 'Mama' a continuation of sucking? This is possible. Has this word a definite significance? I am quite certain that it has not. Of course an association is gradually built up between the mother and this 'Mama'. Under normal conditions it is always reinforced because the father and mother watch out for the appearance of the first word, but when it is not reinforced the word does not take on this significance.

I carried out an experiment which might seem inhuman to you. With my first son, who is now eighteen, I did not confirm the 'Papa' and 'Mama' so that the child has never called us 'Papa' and 'Mama'. Even to the present day he calls us by our Christian names and this horrifies many of our friends. We admit that this is inconvenient. Moreover, we have seen that this experiment may be dangerous because when the child became aware of it for the first time, at the age of three, he had a shock because the teachers were horrified and tried to force him to call us 'mother' and 'father'. This he always resisted. The later children say 'Daddy' and 'Mummy'.

After twenty months the first phrase appears. Although it is difficult to establish exactly when the first word comes, which is not very important in the evolution of the child, the first phrase, on the other hand, seems to me highly important and in direct relation with the general intellectual development of the child. Mentally deficient children will say the first word at about the same time as normal children, but, on the other hand, their first phrase is always delayed. We still have to define what is the first phrase. Most authors agree in considering that there is a phrase when the child is capable of uniting two words in a language. It is then that the true regulating function of language appears: the child filters his phonemes and emotive cries through a web of tenses and begins to organize them.

From two years onwards the number of grammatical possibilities increases with an extraordinary rapidity. Functional normalizations start with the third year. Here again we find this a significant age. Prepositions, declensions, inflexions, tenses, genders, and number appear and there is a change from pre-grammatical language—what might be called Pidgin English (*petit nègre*)—to organized language.

By three years pronouns, past tense, and plurals have been acquired.

By six years, language is almost complete on the verbal plane. We should notice here a certain time-lag: at five years the child can use all tenses and can even use the subjunctive properly if it has been used in his hearing.

There are wide individual variations in the attainment of these functional standards. Naturally, if certain tenses are not used in the hearing of the child, he will not use them either; but he is capable of imitating them and a person who imitates shows by doing so that he

is capable of understanding a situation. At five years, then, acquisition is practically complete. Later we shall find that there is a re-apprenticeship when it comes to written language; this is a new functional level and is of considerable interest because with written language symbolism in the second degree appears. Spoken language is already a symbolism because the word symbolizes the thing. Written language symbolizes verbal language which already itself symbolizes the thing.

It would have been interesting, in order to understand the mechanism of language evolution better, to consider the sequences within the different grammatical categories; but we have not enough time. I will simply recall that 'no' appears before 'yes' and I think this is so for all languages. This is no doubt due to the fact that 'no' has a much greater affective value than 'yes'. Adjectives appear before adverbs, and adverbs of place before adverbs of time. 'Why'—at least in French, though possibly not in all languages—appears before 'how'. 'Me', which is more affective, appears before 'I'. The question of the acquisition of pronouns is worth a study in itself, since it is with the mechanism of pronouns that the child expresses which stage of decentralization he has reached, because the pronoun changes according to the person who is speaking. This is a fundamental acquisition. Here again it is necessary to distinguish between languages because this acquisition varies with the language structure.

It would be worth comparing this language evolution with the different stages of grapho-motricity and with other sectors of behaviour. I think, in fact, that it is up to the psychologist not to isolate one sector but to see how the progressive differentiations occur and to discover, despite possible overlappings, certain functional levels.

We have tried to compare the different aspects of child behaviour which could show the attainment of awareness of self (ZAZZO, 1948). I will only indicate one aspect of our studies, which was the first chronologically. We studied the evolution of language in a young child, particularly of personal pronouns as related to the recognition of his own image in three different situations: in the mirror, on photos, and in films. From this study we obtained some very interesting data. Recognition in the mirror, on photos, and in films is very much delayed when the precaution is taken, in the case of the mirror, of not giving the child any suggestive help or any intensive training. We find that the recognition does not occur until two years and two or three months. It is also found that the recognition of self and of others is interdependent—the recognition of others coming, however, slightly earlier—and this is so for the three situations. With the mirror it is very clear. I have noticed that

although the child is no doubt looking at himself in the mirror he concentrates on the image of the other person because he knows the other person whereas he has never seen himself. There are games in front of the mirror where the child looks behind the mirror as an animal would. Then at about two years we have noticed there is a kind of disorganization as if a sudden state of awareness of self had caused an affective upset. A few weeks afterwards the child recognizes himself in the mirror (at about two years, two months). Up to the end of the third year he displays a certain anxiety and at the same time a certain pleasure in looking at himself. At about two years, ten months this disappears; the image has become familiar and no longer causes uneasiness. However, at two years he recognizes other people on photographs but does not recognize himself until at least six months later.

As for films, he recognizes the situations in which the film was taken; he recognizes the beach or the room where he was, but he does not recognize himself. At two years he recognizes other people without hesitation but he himself is a child whom he does not know. When he does recognize himself at about two and a half years, he uses rather a strange form of speech, as if there were a duplication between himself and the image. He says 'there are Johns' or 'there is a John'. He speaks of himself in the third person.

These phenomena have been confirmed with other children. They always occur during the third year though the exact age varies, and the recognition of other people always comes before the recognition of self.

Now this follows very closely the evolution of language. It is at about three years that the use of 'I' appears without hesitation and is used grammatically. Before that there are no doubt syncretic 'I's'—he says 'I don't know'—but the 'I' is only properly distinguished at about two years ten months. On the other hand, the grammatical and possessive forms 'me', 'you', and 'your' appear simultaneously. The personality crisis with opposition and negativism appears in more or less crude forms between two and a half and three years.

HARGREAVES :

Is there any difference between the recognition age for black and white photographs, which are an artificial convention, and colour photographs?

ZAZZO :

I have not carried out any systematic experiments. I have tried with colour films and the results appear to be about the same, but I cannot give a definite reply here.

Conclusions

Now I should like to draw some conclusions on the laws of child evolution and make some observations on method.

To come back to Gesell—whose work I have drawn upon several times during this communication, without naming him—growth is a unifying concept. From the beginning GESELL (1929, 1945) rejects dualism which, according to him, would make us incapable of properly understanding the liaison between the physiological substratum and behaviour. This can be seen, moreover, in the structure of his tests, where there is no duality, but in any case a differentiation occurs in proportion to age. This is a law of psychological evolution and also of neuro-physiological evolution.

The first general law then would be a law of differentiation and of progressive integration apparent on all levels.

Firstly there is differentiation between various intellectual activities. The studies of PIÉRON (1949) and of his pupils (FESSARD, 1931; MONNIN, 1933), which dealt with scholastic and professional orientation, came to the following conclusions: the correlations between different forms of intelligence are very high at about six to seven years and become gradually lower as the child grows. There are both differentiation and integration at the same time.

On the affective plane as well, we find during childhood a differentiation occurring between self and others, a differentiation which is, at the same time, an interdependence, and which the Piaget school calls reciprocity.

Thus on all planes, differentiation and integration lead the child to finer and more flexible adaptations and to an autonomy which is not an isolation but is, on the contrary, an interdependence and an organization. One could formulate this law in a very sketchy way by using the terms of child psychologists as follows: the child evolves from syncretism and from non-differentiation to a synthetic activity or on the motor plane to synergies and re-organizations of movement.

GREY WALTER:

Dr. Zazzo mentioned the behaviour of children when confronted with a mirror and its use as a test for self-recognition. Now in some of our machines we have demonstrated, quite accidentally, precisely the same type of behaviour. There are certain simple machines which display a characteristic mode of action when they are presented with their own reflections in a looking-glass. It is quite unique, an always-and-only response, so that it is diagnostic, in a zoological sense, of self-recognition. These same machines, when confronted by one another, again display a unique but different behaviour diagnostic of

social recognition; they build up a society which has characteristic modes which can then be interrupted by a common stimulus. The social organization can be broken down into a competitive complex, possibly with the destruction of all the machines. These two behaviour modes of recognition of self and of society can be analysed into quite straightforward cybernetic ciphers. One can describe these processes as being reflexive—I use that word deliberately—and the appearance of them depends upon certain well-defined or definable mechanisms appearing within each individual machine. One can say that if these mechanisms appear, reflexive behaviour will start, it will have its diagnostic characters, and if observing from outside one would say: 'this is a recognition of self', or: 'at this stage a recognition of society appears'. I think this sort of application of cybernetics to these problems can be enormously fertile. It doesn't deny anything (and that's a very important point) but it can affirm a great deal.

I would like to ask Dr. Zazzo one rather trivial question. The use of the term reflexive was quite deliberate on my part—as far as I know it is my own particular use of this word; I don't use retroactive, or feedback, but reflexive. Now does Dr. Zazzo know whether the attitude to self in the child—the time at which it develops, the way it develops, the form it takes—is related at all to the linguistics of the child? In English you tell the child to say 'I wash' or 'I am washing', in French you have to say 'Je me lave'—I wash myself. That is a reflexive verb and in French the reflexive is used a great deal more than the passive.

LORENZ:

In Russian you haven't got a passive, you just use the reflexive.

GREY WALTER:

It often seems quite ridiculous to English people to talk in French about an inanimate object doing things to itself, finding itself, etc. Now, have you any evidence that that affects the way in which a child develops its attitude to itself?

ZAZZO:

At the age when the child recognizes himself in front of the mirror his language is not yet sufficiently well organized for one to be able to see a transformation occurring in the reflexive verbs. From the linguistic point of view we cannot have any indication because these verbs do not exist for the child at that age, that is at about two years two months. On the other hand, we have been struck by the fact that the child begins at this period to use personal and possessive

pronouns and 'me' and 'you' simultaneously. This reply is not very satisfactory because the reflexion you speak of cannot appear.

Your comparison with machines is very interesting, and raises all kinds of problems. How far is the analogy legitimate? In the case of machines, would there not rather be an analogy with mimicry, a reciprocal imitation? What appears to me to be characteristic in the child's behaviour in front of the mirror is his uneasiness. There is a sudden awareness, a hesitation; he sees another and starts to be aware of himself. It is only after several months that this uneasiness disappears and he looks at his image naturally. I see in this uneasiness the mark of a sudden awareness of self which could not have any analogy with the example you gave.

LORENZ:

The disturbance and anxiety of the child when it recognizes itself in the mirror is perhaps based on the fact that somehow the child feels 'Well, I am here, I cannot be there too'. It is really a very difficult thing to understand that my being here doesn't exclude my being there, though it doesn't seem so for adults. But birds do not know, and neither do dogs, that one subject cannot be in two places at once. That's very surprising to us, because to us it is self-evident that an individual object which is here cannot be there. But I know of two chance observations, one in dogs, and one in my pet greylag goose Martina, which show quite clearly that this self-evidence doesn't exist for the animal. I was coming back from a long canoe trip with Martina, and I was dressing and Martina was preening, after a long paddling tour on the Danube. Suddenly Martina gave her flight call and prepared to take off, which was very disagreeable to me, because I wanted to go home and didn't want to lose her. She called again and again, her social call, which was at that time directed exclusively to me, and then I saw what she was seeing. She saw myself, a man in an identical canoe on the other side of the Danube—a fat fellow with a beard. I instantly realized what she was thinking, and tried attracting her attention, saying 'Hullo, here I am' but I couldn't prevent her from taking off. She flew over the Danube and landed in front of that man, and then she recognized that it wasn't me, and so frightened that she rocketed like a firework, and came back to me for protection, then greeted me violently and threatened in the direction of that man.

The second instance is a dog which Heinroth had when a student, a kind of Griffon bitch. Heinroth had a girl friend, and when he went for a rendezvous, the dog knew it, and smelt every lady that looked approximately like her in order to greet her when she saw her. Now when she had come, and the dog had greeted her, this didn't prevent

the dog from looking for other girls resembling her. She wanted another individual of the same kind.

MEAD :

I have a rather detailed study of a child working toward this recognition with films, because I have persistently shown my child films of herself, and I started quite early to watch what happened. She went through the stage that Dr. Zazzo described, in which she would call the child in the film first 'the baby' and then 'that Catherine', which was different from 'this Catherine', and then came the moment when she sorted the thing out. Watching a picture of herself swimming, she was standing with her hand on her doll's perambulator and she looked at the picture and said 'That's Catherine, I'm swimming, that's Catherine swimming', and then she shook the doll carriage, then took up the swimming position, and then said, 'I'm shaking the doll carriage, that's Catherine swimming. I'm shaking the doll carriage', and made the time distinction. This was at about two years and nine months.

ZAZZO :

I should like to spotlight this phenomenon by means of a reverse phenomenon of a pathological kind. I think what is essential here is the formation of the image of the body itself with visual, postural, etc., elements: an image which, in my opinion, is closely associated with the genesis of awareness of self. In certain kinds of pathology disintegration is often the reverse phenomenon of doubling, of positive or negative autoscopv. I am thinking of the case of Guy de Maupassant. The sick man loses his image, or, on the other hand, he may have an hallucination. He sees his own image appearing in front of him. This is a phenomenon which has been frequently underlined in psychiatric literature—and also in other literature. It is a theme which is used, for example, in *The Student of Prague*. We have then two sides to this phenomenon: the establishment of the image of self which is absolutely necessary for the awareness of self, and in certain pathological cases a disintegration where the individual sees himself; one might even allude here to Socrates' demon.

INHELDER :

I have followed with great pleasure M. Zazzo's communication, which testifies to a most discriminating and conciliatory attitude. M. Zazzo said that the Wallon school is often placed in opposition to the Piaget school. I think, as he does, that this opposition is partly the result of a false interpretation of their ideas given by the pupils of

these two teachers. However, M. Zazzo knows as well as I that these two creators of systems can discuss their ideas in all cordiality and friendship, of course, but it is evident that every creator of ideas experiences some difficulty in putting himself precisely in the place of another because above all he defends his own creation. We, the followers, have not the same reasons for being aggressive or egocentric. We are quite naturally drawn to look for means of unification rather than of separation.

I should now like to take up again certain distinctions established by M. Zazzo between the two trends of thought and to add to them briefly.

Firstly, there is a difference in the points of view and the problems raised: M. Wallon raises the question of the origins of all psychological behaviour. This orientation of mind is shown by the titles of some of his works: *Les Origines du caractère chez l'enfant* (*The origins of character in the child*) (1933); *Les Origines de la pensée chez l'enfant* (*The origins of thought in the child*) (1945). M. Piaget is more interested in the end of evolution. He studies *La Génèse du nombre* (*The genesis of number*) (PIAGET and SZEMINSKA, 1941); *Le Développement de la notion du temps* (*The development of the concept of time*) (PIAGET, 1946a); etc. (PIAGET, 1946b).

Next there is a difference in method: M. Wallon seeks, above all, to study the complexity and the clash—frequently tumultuous—of the many factors arising during a phase of development or the passage from one phase to the next. M. Piaget attempts rather to determine the most general comprehensive laws which he interprets as laws of equilibrium. The fact that these methods are complementary is shown in the choice of the facts studied.

The results themselves are complementary. I consider that M. Wallon has made an important discovery which M. Zazzo has perhaps not emphasized sufficiently, that is, the positive role of emotion (WALLON, 1942). Emotion is often supposed to play the negative role of a disturbing agent. Now, M. Wallon has demonstrated the very positive role played by emotion in mental development, facilitating even the formation of thought. M. Piaget, on the other hand, has neglected the emotive aspect of behaviour; he does not deny it, but he has not dwelt upon it because he has attempted above all to determine and circumscribe the structures of adaptation to reality.

Let us now come to the controversy on egocentrism which M. Zazzo underlined. You perhaps know that the discovery of egocentrism on the one hand made the reputation of M. PIAGET (1923) and on the other hand appeared as an obstacle to the understanding of his later work. Personally, I think that the term 'egocentrism' is not particularly suitable. Actually, M. Piaget has attempted to make it more

precise. At present I think he would uphold the following point of view: the social does not exist in itself but as a series of social exchanges maintained by the child with his environment. M. Wallon expressed something analogous by using (as did Claparède and Piaget) the term 'syncretism' which clearly expresses the initial state of fusion existing between the ego and the non-ego. Perhaps M. Piaget did not manage to make himself understood when he called the first social exchanges egocentric relations, establishing an analogy with autism in Bleuler's sense, but it seems to me that the essential point is to show how this first form of exchange is gradually modified until relations of reciprocity and co-operation are achieved. Lack of co-operation due to incapability of seeing things from another's point of view does not in the least preclude fusion with another or imitation of the model. Confusion with its intervening mechanisms of projection and identification is, in fact, an obstacle to true co-operation. Thus, here again it is not actually a question of discord, but rather of complementary interpretation of the same facts.

At the moment I can only see one point on which there might be divergence: the passage from sensori-motor activity to the activity of thought (and we should have to go deeper in our research to discover how much is fact and how much is interpretation).

According to M. Piaget there is a continuous transition between the sensori-motor behaviour of the infant and thought as it develops during early childhood. M. Piaget tries to show through precise experiments (among others, stereognostic tests: PIAGET and INHELDER, 1948) that thought has motor origins. Mental representation is prepared by a whole number of sensori-motor explorations (the mental image being partly shaped by previous tactile explorations). On the other hand, M. WALLON (1942) insists on the opposition between these two phases of mental life. According to him, thought incorporates into itself new elements through contributions from language and social life in general, but this is a matter of hypotheses requiring verification.

Finally, it is interesting to note that Wallon and Piaget seem to have travelled in opposite directions. Each of them has modified the orientation of his research. M. Piaget began by studying the child in his social manifestations, such as verbal exchange. This led up to the study of the child's sensori-motor activities, which M. Piaget is trying to relate to the nervous structures themselves. M. WALLON (1925), taking the opposite path, centred his early work on motor development, whereas now he is concentrating more particularly on the conditioning of the child by the social environment.

This again is a question of complementary evolution and not a taking-up of unshakeable positions.

SEVENTH DISCUSSION

Psychoanalytic Instinct Theory

BOWLBY:

For my remarks I have selected out of the whole field of psychoanalysis the topic of instinct because I feel that instinct is the central core of psychoanalysis, and that the study of instinct distinguishes psychoanalysis from other branches of psychology, which commonly study other aspects of the human organism. Psychoanalysis regards a great deal of psychiatric illness as being due to a disorganization of instinctual life. I share that view, and I think that by looking at it that way we may prevent much mental illness.

Freud made several attempts to formulate a theory of instinct. Both his main formulations were in terms of a dichotomy:

- (a) The sexual and the ego instincts (*circa* 1910).
- (b) The life and death instincts (*circa* 1920).

A further important proposition was that the goal of the organism was to obtain pleasure and avoid pain. Freud himself did not feel very satisfied with his work in this field and in 1915 remarked that it would probably prove necessary for psychologists to look to biology for an adequate theory of instinct. As it happens, in the same paper he outlined a theory of instinct almost identical in principle with that of modern ethology. He conceived of instinct as having a *source* in somatic process, an *impetus* or force, an *aim*—that of ‘abolishing the condition of stimulation in the source of the instinct’—and an *object*, namely ‘that in and through which it can achieve its aim’.

Psychoanalysts have varied a great deal in regard to which of Freud’s theories they have adopted. In Great Britain, under the influence of MELANIE KLEIN (1948), there has been an emphasis on the object-seeking nature of instinct. FAIRBAIRN (1952) has supported this view and argued explicitly against the pleasure-pain theory.

Freud also called attention to the fact that human beings are organisms which at times are driven by forces within themselves which they cannot easily control. We *fall* in love, we *lose* our tempers, we panic, we are possessed by forces which seem alien to

ourselves; and, of course, there is the primitive theory of mental illness that people are possessed by a devil. It is these phenomena that I want to discuss and, just to give them a little more concreteness, I shall illustrate what I have in mind by referring to a couple of patients whom I have seen very recently. One is an adult of about forty, a woman with a severe degree of illness, tremendous phobias, great hatreds, deep depressions, who is subacutely murderous and suicidal. I have been treating her now for two and half years and her relations to me are characteristically ambivalent. I have experienced long sessions when she was shrieking at me for two or three hours without a break. There have been other occasions when her endearments have been on the same scale. I recall that when I came here in June last, on the one hand she was very sad that I should leave her, but, on the other hand, as she told me afterwards, she hoped I would drown myself in the lake—and she meant it. The thing that has struck me so particularly about her is that one day she can be friendly and cordial and we have what I call a sober session, and the next day she rants and raves at me as though we had never had any other relationship. Another feature about her is that she is terrified of friendly relations. She is 'happy' only when she is on angry terms with someone; her sense of stability is better when there is mutual anger. Obviously the problem is, why is she like this? My own view is that this is the result of the relations which she had with her mother when she was small, having been extremely unhappy and chaotic, but, naturally, I can't prove this. What one has *now* is a first-hand and mammoth demonstration of ambivalence: in her case this historical reconstruction must remain very treacherous and unreliable.

The other patient is a little girl of eight. She was rather an attractive, neat, pretty little girl. I asked her whether she knew why she had come to the clinic, and she said, 'Well, I have tempers, but I am learning to control them'. We talked a bit about her tempers, and we got on to her day-dreams; she imagined various things to herself. Sometimes she sees faces on trees—nasty faces—but she turns them into nice faces. Then it appears 'the nasty faces—they want to kill my mother'. From this I had no doubt that *she* wanted to kill her mother—or rather, a bit of her wanted to kill her mother. It's a good family, her mother is a decent woman, but she has tried to smack the tempers out of this child. As a result the child not only wants to murder her mother but these impulses are split off from the rest of her personality.

Now, I give those two illustrations just to show the nature of the problem which we are trying to solve.

I want to remark on three or four psychological processes which may be relevant, and in doing so I shall speak in a hybrid, bastard

language which I have come to use, which derives from both psychoanalysis and ethology. Why do some people develop strong impulses to kill, and why do they actually kill others on occasion? More importantly, since they usually don't kill others—why don't they? That takes us into the field of super-ego development. It has seemed to me for a long time that the controlling force, conscience or whatever we call it in the human being, has an instinctive root. It seems to me that ethology, as I understand it, gives good grounds for thinking this is so; I am referring to Lorenz's observation that the wolves' attacking impulse is inhibited by certain behaviour in other wolves.

Amongst the instincts that psychoanalysts are concerned with are the instinct of flight, the instinct of attack, the following response,* which is of great importance in the study of the young mammal, and, of course, sexual instincts. Now, a very difficult problem is, *why do these instinctive responses become dislocated in infancy?* It appears that the instinctive life of the human being can become dislocated and that it is in infancy and early childhood that dislocation is particularly apt to occur. It would appear that in the early months of life the infant responds to sign-stimuli which are isolated in the sense that the infant doesn't know that they belong to an 'object' in the Piaget sense of the term. Spitz' work on the smiling response (SPITZ and WOLF, 1946) suggests that the infant responds in a friendly way to a particular sort of sign-stimulus; presumably his rejecting responses are evoked by something else. MELANIE KLEIN (1948) has developed the idea of the 'good' object and the 'bad' object; I think she may be using the word 'object' here to mean an isolated sign-stimulus. She has further developed the notion that there comes a point in infant development where the 'good' and the 'bad' object fuse; this I suppose is the same thing that Professor Piaget describes as the 'formation of the object'. It would appear from the psychoanalytic work that the 'formation of the object' is interfered with if there are very powerful and contradictory impulses evoked by the sign stimuli emanating from that object. Certainly, with the first patient that I referred to, the most striking thing about her is this split, the extraordinary independence of her hatred for me and of her affection for me. I can be for her either extremely 'good' or extremely 'bad' but, as an ordinary person, I hardly exist.

The next problem is, if these things become dislocated in infancy *why does the dislocation persist* and affect not only the responses

* The term used by ethologists to describe the young animal's instinct to attach himself to and to follow some other creature, usually the mother. Though it is still a controversial matter, the ethological evidence seems to me to point to its being an instinctive drive in its own right in the human child.

immediately concerned—the following response, let's say, to the mother—but the person's sexual responses and adult love, and even the parental responses of a mother to her own child? In birds at least, these different instinctual responses are curiously independent. Each one comes along, is imprinted to its object, and then another comes along. That doesn't tally with the human, because what psychoanalysts have observed is that in humans there is a horrid continuity, in humans the responses are *not* independent. Of course, one possible explanation may lie in the extraordinary fact (which may well prove a key for psychoanalysis) that a response can be imprinted long before it becomes operative. It may be that human sexual responses are imprinted in early childhood and not later.

A second notion of the ethologists which strikes one is that each instinctual response waxes and then wanes. Each response, ontogenetically speaking, has its own time at the wicket, but in certain circumstances, it would appear that the batsman can refuse to leave the crease! For instance, it looks as though the following response which waxes in the early months of life reaches its peak in the second and third years, and then wanes—and it looks as though that response can, in certain circumstances, persist into adult life.

A third clue to the solution of this problem may lie in the fact that sexual responses can make use of certain components of the parent-child response. It is known that in certain species of bird the male's behaviour towards the female is similar to that of the parent feeding the chick, and the female's behaviour is similar to that of the chick being fed (ARMSTRONG, 1942). If there are certain components in human sexual response which are derived from parent-child responses (which seems clearly to be true) it would not be surprising if problems that had started in the one situation should be carried over to the other.

However, there are other areas that we haven't explored at this conference which may be relevant to this problem, in particular the special properties of responses learnt under stress. The two characteristics of such responses that strike me as being interesting are first that the response is often, in terms of adaptation, an irrelevant response, and, second, that once learnt it has an extraordinarily persistent quality. It looks as though the infant under stress can learn 'silly' responses and the adult under stress can revert to such.

I look upon research on the effects of separating the young child from his mother as being the study of the following response in health and disease, with particular reference to its ontogeny. We ask: in what circumstances is it evoked; in what circumstances does it persist; in what circumstances is it never evoked; in what circumstances is it cut out, or repressed, to use the psychoanalytical term?

Further, what part do hereditary differences between individual children play in these things? These are our research problems.

Further, we are convinced that in research of this kind nothing but longitudinal studies can help us, because we have to have the details of the child's experience, which often cannot be obtained retrospectively even a month or two afterwards. We must observe the child at the time he is having the experience.

Let me conclude by giving you a list of propositions to which most British psychoanalysts would now subscribe (some would add many other propositions):

(1) The dysfunction present in most people with 'emotional disorders' lies in their incapacity to make satisfying relations with other people and in particular with those who occupy the role of love objects to them (e.g. parent, sexual object, child, etc.).

(2) The reason for their incapacity is that the love object arouses powerful and conflicting instincts, e.g. instincts to love and cherish and instincts to hurt or kill.

(3) The disturbed person is unable to tolerate these contradictory instincts so that one or more are divorced from consciousness. One or more are also likely to be inhibited in some degree, though the drive to action persists (or, perhaps more accurately, remains liable to be re-awakened). Such repressed but active instincts manifest themselves in various symptoms and are experienced by the subject as tension, anxiety, and depression.

(4) Such powerful and intolerable conflicts rarely if ever occur for the first time in the relations of the older child or adult towards his love objects. When present in an older person, they are the developmental successors of similar conflicts which were present in his relations to his first love objects, e.g. his mother or his father.

(5) There are special features in the immature human being, especially in the first two or three years, which lead him (*a*) to be particularly prone to strong and contradictory impulses and (*b*) to be unable to sustain their pressure, with the result that processes such as repression, inhibition, displacement, projection, etc., occur readily. This is presumably due to physiological machinery for organization and integration being inadequate.

(6) There is much debate as to why some individuals develop conflicts on a greater scale than others and why some individuals appear capable of sustaining these conflicts in consciousness and others cannot do so. All agree that the child's experiences of satisfaction or frustration in his relation to his parents are of some importance in determining the strength and development of his impulses. Some give much weight to postulated differences in hereditary endowment; some give less.

(7) There is a tremendous literature on the reasons why particular symptoms are developed by particular patients. Few of the theories advanced are entirely satisfactory; some are very unsatisfactory.

(8) There is agreement that the efficacy of therapeutic measures depends far less on their ability to understand and deal with symptoms than on their ability to deal with the primary conflicts in object relations. The technique used is to permit these conflicts to develop within the therapeutic relationship (which they do in some fashion spontaneously) and to help the patient experience the impulses in relation to the analyst, whom he sees and feels as both a satisfying and a frustrating object. When these conflicting impulses are experienced together within a relationship of trust, there tends to be a restructuring of the instinctual life, permitting less conflictual and therefore more satisfying relationships.

ODIER :

I might give you some ideas on the value of affective factors, not only in neurosis but also in the development of the child and the social life of the adult.

Freud and Piaget go up and down the same stream on opposite banks. Each remains on his own side, so they cannot meet. My main idea in my work is to throw bridges between the two banks and my object is to reconstitute the organic and functional unity of child development. On the one hand, there are interesting but partial theories, on the other is the child himself as he develops according to very complex laws.

There is a need, then, firstly to study this development in all its aspects, and secondly to attempt to establish syntheses between the respective data of all the studies confined to one or other aspects of this problem, but I will limit myself to underlining a few points which seem important to me.

As you know, Freud postulated three or four successive theories of instinct which are different and seem to me contradictory. It is these continual contradictions that have helped to disconcert not only biologists but also psychologists and philosophers. This fundamental confusion is constantly reflected in psychoanalytic jargon, and the reader is always constrained to examine texts most critically in order to know exactly what is the subject and on what plane the psychoanalyst is working. Thus, in psychoanalytic works the word 'libido' is used in at least five different senses, without the distinction being clearly indicated. It sometimes expresses sexual pleasure and sometimes the sexual act, and all kinds of behaviour driving the individual to obtain this satisfaction, etc.

There is, then, in these texts a source of confusion between the energy being used, libido or aggressivity, and the instinctual act which this energy is supposed to determine. Thus, according to Freud, instinct is an internal movement from the disagreeable toward the agreeable, from the painful toward the pleasant*; but then, according to this conception, too much insistence is laid on the causal factor and not enough on the intentional factor (it is precisely the latter, if I have understood the situation properly, which escapes the ethologists). Hence these interminable controversies between causalism and finalism of instincts. Now on this subject we can sum up Freud's thought as follows: an instinct is at the same time a cause, an object, and an aim. Here again it is necessary to distinguish between two very different things. According to a first theory the instinct is defined by a tendency to reproduce a previous state which is a state of relaxation or repose. The insistence then is on the actual energetic mechanism of this re-establishment of a previous state, which is a pleasant state, in order to suppress the present state, which is one of tension. Behaviour is then determined by the tension factor, and we have here an energetic conception.

In other texts, however, behaviour is no longer determined by this regulating, almost cybernetic, mechanism, but by the painful, disagreeable nature of the state of tension. In fact, Freud showed that the final state after the relaxation is subjectively less painful than the initial state, hence his famous theory of the pleasure principle.

In short, his first description corresponds to a very strict psycho-physiological conception; his second is a purely psychological approach on a subjective basis, that is to say based on sensations of pleasure or displeasure. The relation between these two forms of phenomena often remains obscure. This no doubt arises from the fact that psycho-analysis is at the same time objective and subjective.

Another subject which seems to me to contain some ambiguity is the problem of the specificity of the object. According to certain theories exterior objects are never releasers of behaviour. They are not the actual cause of instinctive acts. They are rather the effect of a series of selective pieces of behaviour conditioned through heredity and used by the instincts to obtain their satisfaction in the exterior world. Thus instinct as a primary phenomenon is released by internal vegetative, hormonal, diencephalic, or cortical organic excitations. In short, drives or instincts are not initially fixed on specific objects. This has been demonstrated by some remarkable studies on ants (BRUN, 1920). Entirely inadequate ('empty') anti-biological reactions

* This psychic process is invariably started off by a painful tension and is directed towards the final result of a lowering of this tension, that is to say, suppressing pain and producing pleasure (FREUD, 1920).

can be released in ants. I would recall also Dr. Lorenz's experiments on the starling.

It seems, then, that two separate things have been confused in psychoanalysis: the hereditary predisposition to a certain action and the instruments used by instinct either during its phylogeny or during its ontogeny.

Child psychologists, when explaining the search for an object, do not necessarily resort to inwardly determined primary and secondary mechanisms. It seems that from the beginning these mechanisms are intimately linked and that the passage from one to the other is reversible. Thus one finds situations where the object itself can release the instinctive acts and affective reactions which define the first social link, this specific object being the protective mother. This is what is found, for example, in the 'smiling response' which takes on an elective character. Certain babies respond to their mother's smile only. The very interesting problem of the specificity of the object is called in question by ethologists who show that an object is not initially specific. In Dr. Lorenz's films we have seen geese and ducks following him with obvious love as if he were the father or mother. At the moment of imprinting, then, the specificity of the object is lasting and irreversible.

Does the same thing happen in the child? Here is the great difficulty of establishing links between ethology and physiology on the one hand and child psychology on the other. I think that mother-fixation under normal conditions is specific. No other object can replace the mother without producing disturbances which are sometimes very serious and can lead to the anxiety which is called 'the feeling of insecurity'. My work on what I have called 'the neurosis of neglect' (ODIER, 1950) is centred on the origin, nature, and development of this feeling of insecurity in the child and the disturbances caused by it. It seemed to me that emotional reactions played an essential role in the etiology of the 'affects' and that very interesting results could be obtained by analysing in detail not only the instinctive causes of conflict between the libido and aggressivity but by analysing the affectivity itself, and by describing the phenomena not in terms of instinctive causality but in terms of structure.

Affectivity, like thought, has a whole history. Affectivity presents very interesting structures which evolve and are entirely transformed when the Oedipus complex appears, when the child acquires an entirely new conception of social relations based on dualism. In other words, he relinquishes his egocentric position: he can pass from this well-known affective participation, where there is confusion between himself and his mother, to a realization of a new conception, which for boys is identification with the father as an object. The child

begins to feel intuitively that he himself is a subject; he can begin to consider things as subjective. In these entirely new relations the child can be the subject who experiences feelings which are either very ambivalent for his father or loving for his mother, and at the same time he may reverse the situation. It is at this moment that the reversibility of affective relations begins in so far as he can consider himself as the object and his father or mother as the subject. This is a very great problem. Psychoanalytic work has perhaps contributed to spread a certain confusion in public opinion. The Oedipus complex is considered as an instigator of disorder, as a necessary cause of neurosis. In my opinion the Oedipus complex is, on the contrary, a sign of health and of normal evolution. It is therefore necessary to explain to parents that it is a critical stage in development, but of a nature to ensure equilibrium in the child.

This meeting seems to me a fine demonstration of an effort towards synthesis between the biological and psychological planes. I have, however, the impression that the dominant tendency is that of biological reduction, that is, a tendency to carry the actual psychological processes to a level of mechanisms and instincts. But there is another method, the reverse method, which consists in passing not from the more complex to the simpler, but from the simpler to the more complex. By attempting to simplify the complex too much, one bypasses problems raised by the more complex. We now know, thanks to the work of Wallon, Piaget, and others, that the actual psychic processes obey not a causality—which is always very hypothetical—but a legality, a set of rules or laws which it would be interesting to examine.

What is lacking is a link between the two planes in order to avoid what I call the 'jumping method', where one jumps from the biological plane to the psychological plane without always being aware of the jump, and neglecting the whole territory over which one has jumped. It is precisely on an intermediary plane, which I call emotional or affective, that one can investigate what becomes of the instincts spoken of by Dr. Lorenz and others. What is the fate of all this instinctive life? It is transformed into an affective life which leads us to a superior level of intellectual life. It is the affective life which links instinctive and intellectual life. This study of the intermediary plane would lead us to our problem: the development, not only psychological but also bio-psychological, of the child, and would put us on the level of actual experience.

LORENZ :

Many psychological problems have a physiological origin, and there is not a single psychological or philosophical thought which

has not its correlate in the physiological sphere. Now, I should like to see the link between psychoanalysis, as seen introspectively and psychologically only, and the physiological processes. We are concerned with the psychobiological problem. I should like to see the same thing first with one eye, then with the other. Moreover, this is what we must do if we wish to advance.

GREY WALTER:

It is very hard to keep the subject to children and there are several things I want to ask now. First, about the characteristics of the imprinting mechanism, and the I.R.M. that doesn't require a reward or reinforcement, in relation to the psychoanalytic approach to instincts; this seems to me to be of extreme importance. With regard to reward, I should just like to mention an important difference in conditioning between appetitive reflexes and defensive reflexes. In the appetitive conditioned reflex, whether in the flesh or the metal, a reward is necessary—this is the classical association of Pavlov. In the defensive reflex a reward is not necessary at all. A defensive reflex may be self-maintained. In a machine made to imitate a defensive reflex I can show that a single experience may suffice, so that thereafter the conditioned response perpetuates itself. The avoidance of 'pain' or 'displeasure' is enough to make the response perpetuate itself as though it were imprinted, though it is not an imprinting in Dr. Lorenz' sense; it can be shown to depend on the same mechanisms of selection and association that operate in ordinary conditioning.

LORENZ:

Especially if the experience is traumatic.

GREY WALTER:

Especially if it is traumatic. This seems to me to be a very important possible source of error and perhaps also, as so often in these paradoxes, a source of enlightenment, because we may have here the bridge between the conditioned response with associative learning and the type of imprinting which you describe. I should like, as I say, to enlarge on the mechanism of this conditioning process as applied to children and as applied to brain physiology, because I think there is a big field here to study in an objective fashion. It is our ambition to define and describe the instinctive processes in man in terms of quite objective physiological mechanisms in the brain. I think that the study of these learning processes might be extremely profitable to discuss even at this stage.

Another point, which was raised repeatedly by Dr. Bowlby, is the question of affect and what we might call, for the sake of argument, 'reason'; the experience of a number of events, and the sorting out from this experience of what matters. From the physiological stand-point, all behaviour is always a mingling of reason and affect. I have never seen an animal—a mammal, at any rate—behave without physiological signs both of logico-statistical behaviour and of affect. One can show this in measurements and I think a division of: 'this is reason and this is affect', 'you've forgotten affect', 'you've forgotten reason' has no basis in the truth of what happens. I think that certainly anyone who neglects one or the other is wrong; and even if we think we are neglecting one, we are not doing so. Perhaps the psychoanalyst could say why we like to divide things so that we say: I feel one thing in my head and another thing in my heart. I think this pseudo-anatomy is a clumsy, archaic device for indicating the relative proportion of autonomic participation in a behaviour pattern. These are arbitrary and dangerous divisions, and I urge that we should try to insist upon this recognition of the unity of physiological function.

KRAPF:

I think that Dr. Odier has put his finger today on one aspect of our discussion which is particularly close to our subject-matter when he introduced those two polar conceptions of causalism and finalism, because there it would seem to me that what he and Dr. Bowlby said links up with what we heard from Dr. Zazzo and from Mlle Inhelder, about the psychobiological development of the child conceived in terms of the socialization phenomenon. I would like to take up something which has been said by Dr. Bowlby, that conscience has an instinctive root. Obviously conscience is a phenomenon of socialization, and I would be in full accord with him if he said that conscience had *also* an instinctive root, because it is questionable whether the genesis of its function can really be referred exclusively to an instinctual origin. This leads us into the central problem of the super-ego. When Freud defined the super-ego for the first time, he did not immediately introduce the term super-ego, he spoke of an ego-ideal, and later on he changed over more and more to the term 'super-ego', and 'ego-ideal' gradually disappeared. But many years ago Dr. ODIER (1926, 1943) wrote a paper in which he suggested that conscience had in fact two roots, one which could be described as 'super-ego', roughly identical with Freud's 'ego-ideal', and the other, which he called the 'super-id', with deep instinctive roots, which would have been associated with the super-ego of Freud's second

conception. Now, it would seem to me that Freud's second super-ego, Dr. Odier's super-id, would be what Dr. Bowlby had in mind when he said that conscience had an instinctive root. It seems to me there is another factor in what we usually describe as conscience which has a finalistic structure, and this corresponds to Freud's ego-ideal and Dr. Odier's super-ego. In fact, I believe that this difference has ramifications even into such finer psychological points as the moment when the super-ego is born. Is it true that the super-ego is purely the heir of the Oedipus complex, as Freud supposed, or is it developed at a much earlier stage, as Melanie Klein says, or are there two forces in the determination of behaviour, one which is causal, and tied to instinct, and the other finalistic and defined as motive? I would very much like to hear what Dr. Lorenz has to say about this: whether there might be a difference between man and animals, the animal perhaps being finally directed in a biological sense, but not in the sense of a conscious finality.

ODIER:

Concerning Dr. Krapf's remarks, I should have liked to speak of the super-ego and the structure of the super-ego mechanisms which are manifested in the very frequent symptoms of what is called auto-punishment. In his famous *Memoirs* of 1923 Freud exposed his concept of the super-ego, but he did not differentiate between various expressions: the super-ego such as we find it in neuroses, the ego-ideal, the conscious moral conscience, if I may say so—because it would be frightful nonsense to speak of unconscious moral conscience—and finally the ideal of the ego. It is on this subject that I published the study (ODIER, 1943) alluded to by Dr. Krapf to show that in fact four different things were concerned. A distinction must be made between what I call pseudo-morality of neurosis and a new form of morality linked with motivations from moral conscience which itself is linked with what I have called choice or adoption of norms because our moral life always has a normative character whatever the value or the nature of the norms we adopt.

KRAPF:

This can be envisaged as a socializing process, can it not?

ODIER:

Yes. We must evaluate certain aims which determine and stimulate our work and our interests, etc. Now this process of evaluating aims is very little understood. To my knowledge no work on the subject

exists, although it is at the centre of social life and the evolution of the human being.

Another point on which I should like to give some details is that of the difference which I thought I had noted between our results and the data of the ethological school. What do we find in animals? A lack of selectivity. They can follow anybody as long as the imprinting occurs sufficiently early. Among infants, on the other hand, there is an excess of selectivity. The fact that many children have a mother-fixation is determined by a factor which has certainly escaped the physiologists and ethologists and which nevertheless plays a fairly big part in human life. This factor is the need for security which is conditioned by the need for love. What, for example, do you think of the concept which has been called the instinct of security? I think that this raises a big problem. In my opinion the hypothesis that such an instinct exists is useless because I think that the need for security is the consequence of a series of experiences in the child. In certain states the child experiences very agreeable feelings and in other situations, on the contrary, he feels anxiety. Hence his need of re-establishing the feeling of security by elective mother-fixation.

Generalizing, then, from this point of view, one might say that the infant experiences two kinds of feelings which it is difficult to find in the animal: on the one hand fear of his instincts, which is why he represses them so energetically, and notably fear of his sadism (the English school now postulates the congenital nature of sadism, children being supposed to be born with a strong sadistic tendency); on the other hand, there is the fear of the exterior world, the fear of frustrations and menaces. This fear leads him to see in the exterior world hostile forces which he attempts to explain to himself by magical thought, animism, etc., by all those forms of thought so well described by Piaget. Many children, in fact, see in the exterior world a baleful influence which constantly menaces their equilibrium and their life: they therefore attempt to defend themselves by tightening the exclusive link with a specific object—the beneficial protecting object—the mother. From this arises the very common conception in affective life of 'all or nothing'. And this certainly is an inevitable source of deep frustrations and great deceptions.

LORENZ:

If you will pardon my saying so, I think that in the discussion of the last few minutes there has been a horrid mix-up regarding causality and finality and regarding the subjective and objective side of behaviour. I will start on finality. I should assert that 'finalism' is as much a nonsense as 'causalism'. Let me explain in a parable. I

am driving along in my old car, with the finality of giving a most important lecture. I am enjoying the wonderful finality of the construction of my car, how beautifully all its constructive details are calculated to help humanity to be enlightened by my lecture—it's the finalists who are apt to be so assuming, because I am not. Now, suddenly my old car says 'hp! hp! hp! hp! hp!' and stops.

And now I'll find out something fundamental. I'll find that finality is not a force. It cannot be used for traction. It is causality that is pushing along my car—and everything else on this planet and all others. The fact that my intended lecture is of the utmost importance for the salvation of humanity—in other words, the value of the goal—does not help me three whoops in hell. The only 'factor' which can direct my activities in the right direction is the insight into the causality of the defect. It may enable me to screw out the jet of the carburettor and blow out the drop of water that caused my motor to peter out.

Let's take another example. The finality 'factor' won't help a man in whose appendix a cherry stone has got stuck, but the youngest pupil in the surgical clinic can help him, if he has insight into the cause of the illness. And that is exactly why medicine, the queen of all applied sciences, is forever occupied with the unravelling of the causes of all illnesses. It seems to me very futile to quarrel about whether the question of causality or the question of finality is more important. Neither is of any importance without the other. The investigation of causality would be quite senseless if humanity were not striving for goals. And humanity striving for goals would be powerless if it had no knowledge of causality, which gives it power to change, at its will, sequences of natural events. This relation between the final and the causal question must be grasped; it is perfectly simple. Yet finalistic behaviour students persist in accusing ethologists of being blind to the finality of things, and they do so in spite of the fact that we actually begin most investigations with a quest for finality. When I see a queer organ in a new animal, I do not start with any 'causing' investigation, how the animal got to have that organ, but I start with the question, What is this organ for? And even in putting this question, we must not forget that the whole conception of finality can only be applied to processes moving in a certain direction, which certain processes in the universe do. We must not forget that the finality of adaptation in the organic world is an arrow of direction which we have fixed to the process post factum, and we don't see the many abortive trials of nature, the whole vast quantity of animals and plants that have become extinct.

Now as to finality in behaviour, I think I will have to explain something very commonplace which concerns appetitive behaviour

and the consummatory act: one of those commonplaces which are so incredibly difficult to see. An animal's behaviour is directed; it is purposive, and EDWARD CHASE TOLMAN (1949) has given a very good objective definition of purposive behaviour. Purposive behaviour is characterized by the fact that the same constant end or goal is achieved in the animal by variable adaptive behaviour. Now all finalists, most prominent among them McDougall's (1933) school of purposive psychology, had assumed, more or less *a priori*, that the end or goal for which the organism is striving is nothing else than finality itself, in other words, the survival value or the biological effect of behaviour. For a school of thought which regarded Instinct (with a capital I) as a 'directive factor', it was entirely consistent to assume this. The great discovery of Wallace Craig was simply this: the organism as a subject does not strive for the biological effect of its instinctive activities, but only for the discharge of these activities for their own sake. It seems utterly commonplace that I do not go to luncheon because I purposely want to get fatter, but because I like eating and shall become most dissatisfied if I do not get food. Also it seems a rather rude and crude *reductio ad absurdum* to state that the young man does not try to impress the beautiful maiden with the direct aim of becoming, as soon as possible, the father of a lustily squalling baby, but for very different reasons. I think that in judging and describing animal behaviour, we cannot be strict enough in keeping apart the survival value of behaviour and the subjective end which the animal or man is striving for. We are quite justified in saying 'this animal is striving for an end'. If a dog wants to kill the rabbits in a rabbit-hutch and first tries to dig a hole under the rabbit-hutch and then tries to jump under it, and then finally gnaws a hole into the hutch, then that's variation with constant end.

Well, so much about finalism and causalism. There ought not to be one single biological investigation which doesn't take both viewpoints into account.

BOWLBY:

It is interesting that Freud made this distinction in his 1915 paper on instinct (FREUD, 1915). And this notion is, I think, crucial to the whole Freudian theory of instinct—that the aim of the instinct has nothing directly to do with survival.

HARGREAVES:

I was much impressed that Dr. Lorenz' wording is almost the same as Freud's; instinct has the aim of 'abolishing the condition of stimulation in the source of the instinct'.

LORENZ:

Dr. Odier said that the selectivity of the human child in regard to its mother should be so much greater than the selectivity of an animal directed by I.R.M.s. Now I am going to ask him how does he know? Has he ever tried to offer a simplified or otherwise different substitute mother to a newborn child without previous experience? And, of course, nobody would dare to make the experiment, to try a chimpanzee mother and let her foster a human child. And I can tell you what would happen: she would drop it very soon. I am not prepared to accept the statement that the human child's reactions are so selective. On the contrary, it is our general experience that the I.R.M.s of the higher animals are much less selective than those of the lower animals, because more scope is left for learning.

HARGREAVES:

It seems to me that both the statements are true because the Spitz smiling response is very unselective. It is not a smiling at any particular person: it is the smiling at an oval object with two black dots on it which is moving. But within a comparatively short time—I think about a month—the response is given only to the mother.

LORENZ:

The greylag goose gets imprinted to man within ten hours, and it will know its mother within about forty-eight hours, and not respond to any other goose. It is very curious for the narrowness and specialization of its Gestalt perception that the little goose finds it much easier to learn the difference between the individual facial expressions of two geese, than between two human beings. One of my students, Margret Zimmer, and I each reared flocks of geese this year. We consented to 'peck' at each other's children, and even with this reinforcement it took the goslings about three weeks to differentiate clearly, to know the two of us apart. Miss Zimmer is rather a petite blonde girl of twenty-four and, well, you know me! Yet it took three weeks until her geese wouldn't follow me any more and mine wouldn't follow her, while you never see any attempt of a little goose to follow another goose except its mother after two or at the utmost three days. As to Dr. Odier's question of 'l'instinct de sécurité', my answer must refer to the difference between McDougall and the purposive psychologists, and us. The purposive psychologists who were prepared to answer the question 'why' with 'so that' had naturally no compunction at all about giving instincts names according to their ends and creating as many instincts as there were ends, whereas our concepts

of instinct are based on physiological mechanisms. As to the instinct of security—well, the goal of security may be reached by any number of neural mechanisms which all contribute to that end.

I want to add one point to a question of Dr. Bowlby's which interested me very much because it is our chief object of investigation just at present: that is the overlapping and interacting of two simultaneous instincts. We thought formerly that instincts in animals were mutually exclusive. JULIAN HUXLEY (1914), speaking in parables, said that man resembled a ship with many captains on the bridge fighting all the time between themselves, while an animal was a ship which was also governed by a number of captains, but when one captain popped up in command on the bridge, the others had to go below. I have quoted that again and again because I believed it myself. But it is not true in the least. One excuse for thinking it was our choice of examples. We were quite justified in seeking the most simple examples, just as Mendel searched for the monohybrid type, but in reality we find that these mutually exclusive, unmixed instincts are about as rare as hybrids heterozygous only in one gene. Everything can overlap with everything, and the result is very often a quite crude superposition of movement just as two automatisms can be superimposed on each other in one muscle contraction, so you can have two instinctive movements superimposed. In my film of the courtship of the mallard duck, you will see a very beautiful example of superimposed instinctive activities. A female mallard is trying, simultaneously, to execute the movements of inviting her male to copulation and inciting him to drive away another drake who comes disturbingly near. At first you see her doing the two movements—both are movements of head and neck—alternatively, and then the two rhythms begin to attract each other by what HOLST (1936) calls 'Magnet-Effekt', until both movements fall into step and form the queerest superposition.

Our assumption is that superposition is the most primitive form of interaction between two instincts, and that mutual inhibition is only due to a secondary mechanism which evolved later. It is, of course, very necessary that the instinct of escaping blocks all others, or that sexual instinct prevents fighting or killing the mate, etc. It also may be regarded as an argument for our assumption that in pathological cases, in which the inhibiting and dividing superstructure gets defective, there occurs a mixture and superposition of instinctive movements which normally never occur simultaneously. If you get down to the epileptic fits, where all superstructure is suddenly struck away, you get a cacophony of pretty well all the endogenous movements that the human has—escape activities, sucking activities, etc. But your description of that woman who hated, Dr. Bowlby, and at the

same time was affectionate to you, strongly reminded me of what some fish will do, which primarily are aggressive, but in which the aggression has to be inhibited in order to let the pair take care of the brood together; this inhibition mechanism is very fragile and very easily broken down, and you suddenly get an awful fight between the couple.

EIGHTH DISCUSSION

The Cross-Cultural Approach to Child Development Problems

*Films presented by Dr. Lorenz and Dr. Mead**

MEAD :

I want to emphasize two or three things on method first. The approach I will be talking from is based entirely on the study of a whole community whose place in a series of communities constituting a closed culture is known. There may be seven or there may be seventy villages, but before one selects a village one knows its size, its composition, its relationship generally to the whole of the pattern. I make the choice of a village that is itself as closed as it is possible to get (which depends on the society), that is clearly demarcated and contains a functioning whole ranging from grand-parents to babies. I don't study any village of more than 500 persons, and I prefer a smaller one. I study the whole community so that I know the positions and relationships of everybody in it. If I concentrate then on particular children or particular families, I know their place in the whole structure. If I concentrate on particular pieces of behaviour, such as that of the pre-school child or of infants, it is against the picture of the whole. It is as if you threw a light on the whole tribe and then a brighter light on a smaller group, and a brighter light still on a still smaller group. For instance, I have a thousand photos and ten thousand feet of film of one of the babies in a Balinese village;

* Films presented by Dr. Mead: *Bathing Babies in Three Cultures*, a comparison of the interplay between mother and child in three different settings—bathing in the Sepik River in New Guinea, in a modern American bathroom, and in a mountain village of Bali in Indonesia. Part of *Character Formation in Different Cultures*, a series of films produced by Gregory Bateson and Margaret Mead, on the basis of field work in Bali and New Guinea, showing the relationships between the way in which infants and children learn their culture and other aspects of behaviour, particularly dance, trance, and other forms of dramatic behaviour (New York University Film Library).

of many other babies I have perhaps only fifty photographs. By using photographs and film as well as notes, we preserve all the things that we don't know enough about to look at, and all the things that are going on simultaneously so that one could not possibly write them all down. It is possible with these new techniques to carry many more unanalysed variables than ever we could before.

It seems to me that we are working here in this group with two problems. The first: trying to define the nature of maturation for human beings. What are the minimum points below which it is not possible to learn certain things? What is the whole pattern of chronological maturation and the degree of individual differences? Do these individual differences fall within a normal curve of distribution, or are there extreme temperamental discontinuities that are innate? My expectation is that in the end we shall decide that there are a large number of innate individual differences in every society; we have no reliable method at the moment of measuring any sort of constitutional differences that are cross-cultural. My impression after working intensively in seven primitive societies is that the range of temperament in them is about the same as it is in our own society. The range of intelligence is usually narrower, but that is very largely a question of the small size of the group, and the fact that most of the defectives do not live, so that you have less at the lower limit; and your chances of a genius are enormously reduced when you are working with a people of whom there may be fifteen hundred in all.

Now the other problem we have to deal with is the role of culture in patterning the growing individual. When the anthropologist works with one of these communities he first makes a cross-sectional picture of the adult culture—the finished mature behaviour, the institutional patterns, the language. Then he studies the way individuals are inducted into the society and learn the culture. I would expect that when we get to the point of being able to specify the pattern by which the individual learns the culture and the pattern which is represented cross-sectionally in the adult you would recognize them as very highly identical patterns. This would only be so, of course, in a static society which is not changing, where the adults have been inducted in just the same way as their children are being inducted. But in a society such as the Iatmul of New Guinea, where the grandparents have been held by the arm in exactly the same way as the baby is held today, so that every member of the society of every age has experienced the same infantile situation, you expect the old adult to represent the entire growth sequence and growth experience, and to represent it to the young individual.

We can make distinctions between societies where children are reared by their grandparents, and societies where they are reared by

their parents. In societies where they are reared by their parents, it looks as though they develop more curiosity and more adventurousness, and they are less conventional. In societies where they are reared by grandparents, they learn as babies what it is like to be old, and accept it. A third possibility is where children are reared almost entirely by other children, carried around by little girls and boys or just by little girls; this has certain characteristics in common with rearing by peasant nurses in that it keeps the child much closer to its own bodily processes than when the rearing is done by an adult.

We assume that the cross-cultural differences of mother-child behaviour between New Guinea, Bali, and modern America and all that they represent in terms of technology, attitude toward life, posture, gesture, etc., are learned behaviour; that there is nothing in the Balinese that prevents them from becoming as awkward as Europeans. Within one generation of rearing in Europe of Balinese babies, you would expect to lose the lovely weaving of the fingers. I have had one experience of an American child in Samoa, where the dancing is not as beautiful but is somewhat of the same order. At two he could dance beautifully in the Samoan style. He came back to the United States at three, and at ten he was as awkward as any American child. The whole thing had probably not been eradicated but driven underground, and when I spoke Samoan to him he sniggered as if I were saying something pornographic. He has an IQ of 160 but a deficiency in written English, and when I analysed his English it had Samoan grammatical locutions underlying it. This rather dramatizes the points that children from quite different environments can learn these things as infants and also that these things are all learned. The Balinese hand is like a monkey's hand. In America, when I stand in a line and shake hands with college students, about one in two hundred has a hand that is like a Balinese hand, and if I ask the girl what she hopes to do, most probably she answers, 'I am going to be a dancer'.

On the other hand, if we look at the often exceedingly elaborate ritual styles, we find that each culture has picked out the innate potentialities of part of the human race and, because of our great capacity to learn, has devised ways of teaching the other members of the culture to do that particular thing. There is a good deal of evidence (of the sort that underlies the writings of Jungian students) that there are a limited number of these characteristics that recur sufficiently often so that we will have repeats of psychological patterns in many different parts of the world where we cannot prove any historical connexion. Myths (which as nearly as we can tell are quite independently evolved by different peoples) are an example. They cannot simply be referred to childhood experience, because some peoples don't have them. One people will develop

them and another will not. There is a possibility of recurrence in every society of types sufficiently alike to account for these similarities, and then each society takes one or more for its model. For example, sometimes a society will pick one temperament for a man and another for a woman. Another society will pick the male and female of the same temperament, and another society will distort one sex greatly in order to bring them into accord not only with the temperament, but with the sex of another group. In very complex societies, such as modern England or France or the U.S.A., we have, of course, class typing by learning, and we have occupational typing. We have our Bohemias and our Greenwich Villages, which are socially selected mechanisms for people who find one way of life more acceptable than another. In primitive societies the range is very narrow, and everybody who is going to stay alive has to be fitted into a tighter pattern.

Every human culture has to be learnable and usable by everybody who is going to survive in that society. You can have a society that keeps only half the babies alive; there are very many who do. You can have a society that says that people who are born deaf aren't going to talk, and they are put in an outcast position. But if you take the position that is developing in the Western world, that the people who are born deaf are going to talk, you may then have to try to modify your linguistic usages so that people who are deaf *can* talk. In a society which has a language that makes lip reading difficult, and which also has a large number of deaf people, one would expect that in time the language would be modified. If you insisted on Braille and had a large number of blind people, then you would have to work out a machine that would recognize the letters, or you would have to alter your script to make it more adaptable to Braille. This is just an extreme example of the point that everything that is learned has to be learnable by every ordinary human being in the society, in addition to being exceptionally congenial to some temperaments. If we all sat here and thought what kind of language we would make up if we wanted to make one up, our ideas would differ, but if we wanted to use the language, it would have to be modified so as to be learnable to every person here. In a sense every culture is a model of every other culture and contains all these possibilities, which is why an adult can move from one country to another and continue to operate. The place where we can conventionalize that is language. Man has known for thousands of years that he could learn the language of another group. We haven't conventionalized the rest of our cultures to the same degree, so many people will go into another society and learn nothing but the language (not even the postures and gestures that go with it). But if we take language as a model, it seems that any human being who has thoroughly learned his own culture and learned

that cultures can be learned, can learn another as an adult. And that learning is of a totally different character from the learning as a child.

HARGREAVES :

What about the sub-cultures of occupational groups ?

MEAD :

Well, they can be learned if you learn that you can learn them. People in the natural sciences are generally taught that other people can't learn them. The scientists say: 'They will never understand; they can't count.' In a class society, the members of the lower class are taught that they can never learn to be members of the upper class, and if they are taught that well enough the class society will last for a very long time. So when one says that every culture is learnable by every member of every other culture, the statement contains all of this 'learning to learn' in it. HEBB (1949) discusses in his *Organization of Behavior* the probability that first learning is of a very different order from adult learning. Even if so, however, each culture contains the record of that first learning in its adult institutions and patterns.

I want to say something about the application of some of Gesell's work cross-culturally. The book *Growth and Culture* that Frances Macgregor and I collaborated on was an *ex post facto* attempt to do this (MEAD and MACGREGOR, 1951). I had not been very much interested in Gesell's work until in 1945 I saw what Frances Ilg was doing with it, with her enormous capacity to assess kinaesthetic and motor development. We worked with four thousand photographs of eight Balinese children on whom we had accurate age records. (And they were the only infants on whom, in two years' field work, I had been able to get *both* dates of birth and long sequences of photographs—this in a society that keeps accurate dates for the first seven months of life! I wish everyone who has comments to make on material in primitive societies would bear this in mind—that it took two years to get sequences on eight children whose date of birth we knew with absolute certainty.) We worked with these photographs along with the whole Gesell group, spreading the photographs out in various simple categories and letting the Gesell people pick out from our children the things that struck them as completely deviant. Then we took their categories and studied them. The general finding was that the gross chronological stages were identical in the two cultures. It has been my general feeling (working always with approximations and very few real ages) that the sort of thing that Professor Piaget's and Professor Wallon's school are agreeing on in periods from five to seven or the period about twelve is reproducible cross-culturally.

What we found was this sort of difference. The Balinese child's hands and fingers are very delicate. The lovely type of precision that one gets with the Balinese is an earlier development than our type of prehension and is accompanied by very poor thumb-forefinger opposition. When the child picks things up it opposes its thumb to the second and third finger and not to the first and tends to bring the fingers down to the thumb rather than bringing the thumb toward them. The sort of thing that we think of as using the thumb is under-developed, and there is a high development of the ulnar side of the hand. Prehension gradually develops, and the children pick things up with the third, fourth, and fifth fingers, and there is an emphasis that gives a tilt to the handling of things. In the same way, the development from crawling to sitting differs somewhat. American children go through a long period of either hitching or crawling, walking on all fours, then they stand, and much later learn to squat. The Balinese child is not permitted to crawl very much and is always being picked up because crawling is behaving like an animal. It goes from sitting to squatting to standing. The squatting is a much deeper and broader posture and as nearly as we can tell is an effect of being carried on the hip but not having to grasp because of the sling, so that the child just spreads. The mother just hangs the baby up on her; or there may be a child-nurse who plays violent running games with a baby in a sling around her neck. The child has to learn to adapt to this. If it didn't, it would have its neck broken, and conceivably an occasional baby does get its neck broken. So apparently a combination of the cultural attitude about animality which the Balinese are apt to stress (one tries to keep one's animality under control) and the method of carrying babies produces a posture which is a different kind of squat from ours.

FREMONT-SMITH:

Are there vipers or any other dangers to crawling?

MEAD:

No, these are urban people. The prohibition is directly related to the feeling about animality and incest. People who have committed the crimes that are most disapproved of, bestiality and incest, are made to crawl on the ground like pigs, eat from a pig's trough, and wear a pig's yoke on their necks, in expiation for their behaviour, before they are banished from the high gods and condemned to the vicinity of the cemetery for ever.

I want to stress, for a minute, the interaction between the mother and the child that we get in these living societies, that gets left out

here when one keeps talking about 'the child does this', and 'the child does that'. A Balinese baby is limp and soft, but at the same time capable of performing quite elaborate acts even so young. The teaching is kinaesthetic (you get behind the child and you teach him by manipulating his body) or it is visual—never verbal. The Balinese are incapable of learning anything verbally. They are incapable of carrying out the Binet-Simon Grade III instructions. The Gesell phrasing of it is that the child keeps the neonatal flexibility that we see in very young babies. You get the same kind of weaving flexibility; you see it in Spitz's films on regressed, neglected hospitalized babies—the wandering hands with each finger going off on its own. The hand postures are exceedingly asymmetrical. In Bali any part of the body can go into trance. The little finger can go into trance and so can the whole hand. There is catalepsy of parts, and there are ceremonial trances in which the hand goes into trance but the rest of the man doesn't. The assumption is that you have here a mother who expects her baby to be limp, who embodies in every movement she makes in her handling of the baby an expectation that the baby will continue to be neonatally flexible. The baby, then being flexible, again reactivates the mother's expectation of flexibility, and the two things interact. If you could give a baby of this sort to a Western mother, you would alter her behaviour to some extent, especially if she were an experienced mother and could feel the surprise in the baby. On the other hand, if you gave an American child of nine months to a Balinese mother, it would probably learn a good proportion of this behaviour. My suspicion would be that the one girl in two hundred that I encounter in a group of American students whose hand feels Balinese has some hereditary predisposition that is like the hereditary style that the Balinese have made into a whole culture that is learnable, and learnable at different stages.

I want to make one remark about thumb-sucking, because I think it fits into what Professor Lorenz was talking about in connexion with I.R.M.s. One of the most astonishing results is that we have never found thumb-sucking in genuinely primitive babies that haven't had any Western-type public-health nurses around. Even in societies with enormous oral emphasis where they suck everything else—their wrists, their lips, their knees—they never suck their thumbs. That does not mean a thumb never goes into the mouth, but a typical thumb-sucking position with the thumb deeply in the mouth is absent. The only explanation of this that I can suggest is the kind of suckling the child receives as a very young infant. Just once in my experience have I seen a baby put its thumb in its mouth in the thumb-sucking position—at about two hours after birth, when there was no one there to suckle it.

FREMONT-SMITH:

There is evidence that thumb-sucking probably takes place in utero. Swallowing of the amniotic fluid certainly takes place and the thumb is in a very convenient position for getting into the mouth. At Caesarian operation the thumb has been observed to be in the mouth.

STRUTHERS:

One sees newborn babies as they come from the delivery room, before they get to the nursery, who are sucking their thumbs.

MEAD:

I am suggesting that thumb-sucking represents some sort of later phase of deprivation, that if a baby does not have enough breast-sucking at a given period it will take to it again, but properly breast-fed babies are babies that are fed a great deal and fed when they are hungry and without that long period of wait at birth. The thumb-sucking is absent in every group where the baby is fed by somebody within an hour of birth.

I would like to close with one other general point. We have a great deal of evidence from clinical material that man is capable of receiving exceedingly specific irreversible impressions. The sort of thing that is called in technical slang 'a lech' has the type of specificity that is extraordinarily suggestive of some phenomenon related to imprinting. A 'lech' is the sort of sexual demand that makes a man go to a brothel because he wants something exceedingly complicated. He must have a woman in a white dress with a red sash with spots of ink on it, or something. Usually his demands contain elements which are uncongenial to almost all wives. It is the sort of thing that the brothel seizes on.

We also have evidence that human beings are capable of making self-selection of foods that are right for their own growth; experiments have been done on rats that show that a rat, confronted with prepared substances which never existed in human history in a pure form, can pick from among them a better diet than a scientist can choose for the rat's particular needs (RICHTER *et al.*, 1948).

HARGREAVES:

But it has been suggested that it is a learning process. The first try makes you feel better than the others and therefore you concentrate on this.

MEAD :

What I meant is that this is a potentiality, whether it is completely innate or learnt.

FREMONT-SMITH :

It needs one thing to be added to that, which rather dramatizes it. The rat when pregnant will shift to a diet that is appropriate when the infant rat is born, and it shifts again when the weaning takes place. And when the adrenal cortices have been removed the rat readjusts the diet promptly, taking in enough salt to keep himself alive from a salt solution which is available alongside the fresh water, and which he ignored previously. A similar thing is true of the rat made diabetic. The rat shows an extraordinary capacity to adapt himself to his bodily needs in a way which is very hard to conceive of being learnt. Of course, it has survival value and in that sense may make him feel better.

MEAD :

We know that human beings will do some of these things. There are cases of children without an adrenal cortex who could not yet speak, who have learnt to eat salt. There is a very particular case of that, an instance in which an adrenal-deficient child had learnt to eat salt while the mother was sick and the mother did not know of this. The child was taken to hospital, had no way of finding the salt, had no way of saying that it needed the salt, and it died.

Now the difference between building a diet which will keep most people alive comfortably and properly fed, and creating a situation within which each person can self-select the exact items which he needs, represents one of the dilemmas which society has always dealt with. It looks as if it is wiser to create a more generalized pattern and have people learn. This can be generalized to the point where we do not coerce individual idiosyncrasies, where it is possible, for instance, for children to refuse to eat something. In some societies children are forced to eat things and some of them probably die because of it. American share-cropper children are forced to eat fat; they rebel violently but unsuccessfully and that gives them an inability to learn to eat meat later on. Something has evidently been forced upon them that is nutritionally so bad that it creates a learning defect. In every culture you see a compromise between the finding of a pattern within which all individuals can survive (though with different degrees of free activity), and the insistence on a pattern that is lethal or destructive or deforming to some individuals.

LORENZ :

I would like to ask a question. First, I think that we ought to ask whether the special requirement in rats leads to the choice of that unknown food factor at the first time, or whether it is found by trial and error (which I strongly suspect). I think that the answer is enormously important, because we know of cases where there is a most specialized I.R.M. present for such a special need, for instance, the I.R.M. for chalk in birds. Its optimal object simply consists in something hard and white, but just not too hard to be nibbled. Anything which fits that I.R.M. would be eaten by birds in need of chalk and I have killed valuable birds by giving them calcium carbide, which I mistook for chalk. It just shows that there may be an I.R.M. which is never used in the wild bird.

Now I can go on with what I have to say. Different people say that there was a tremendous resemblance between Dr. Mead's film on babies and mine on birds. Now I want to give a warning; you saw a similarity but you did not see the same thing. You saw something analogous, but on a very different level. In both films you saw behaviour patterns which have evolved historically. But in one case they evolved in the palaeogenetic history and in the other in the cultural history. Both ways of evolving differ from history in the common sense of the word in being much slower and much more conservative. This forms a link between the two, and it is this conservatism in how you court, or how you hold your child's hand, or how you dance, and so on, that is responsible for the tremendous similarity, because it produces ritualization. In my opinion, the analogy between ritual dances and ritualized instinctive activities lies in two points: first, activities which primarily had mechanical function, like sowing, or reaping food, etc., have been in turn made into something with an entirely different symbolic function. Second, several independent movements, joined on to each other by an adaptation to the plastic needs of the situation, have been welded together into one rigid form, which, by that very process of welding, achieve autonomy, as one pattern of behaviour.

MEAD :

I agree with the welding point, but I would question the slowness of the process. In an unpublished study on hand postures in courtship (in the biological sense rather than the social), among young people in Louisville, Kentucky, Ray Birdwhistell distinguished over twelve ritualized pieces of behaviour in the way the average college boy in Louisville holds a girl's hand. They are stylized and ritualized and not articulate.

LORENZ:

Are they not a fashion?

MEAD:

No, they are not a fashion in the sense of being articulately communicated; but I don't have any reason to believe that they are a hundred years old. They are systematically linked with so many other things in the human body; the body as a whole is a factor in the ritualization. In New Guinea we have no indication at all that some of the cultures are more than five or six generations old. They change with extraordinary rapidity and change into very different styles, with the human body as the integrator.

LORENZ:

Gesture is a convention; it has to be learned. There are innate elements in it, though. Your directing your hand towards me so that everybody understands your meaning is a convention; but it evolved historically from a series of innate movements.

HARGREAVES:

- But you could not call the stiffening of the arm in anger a convention. It is a conventionalized use of something which originally arose in another situation.

MEAD:

The New Guinea temper tantrum gesture, you mean? It is absolutely characteristic of children in that part—it occurs over and over again. You can say that babies learn to do that because they see other babies doing it. You can say they do it because of the way the mother holds the arm. It can be learnt very fast though. That is an important point which I think we should go into here, because in cultures like modern America the speed of change is so great that there is very little ritualization.

GREY WALTER:

I want to ask if anything is known about the extent to which the ritualized gestures, either in birds or lower mammals, or in human culture, could be related to some complex of physiological mechanisms. These are things about which we know very little, but which are described in great detail in some of the Eastern cultures, particularly in yoga, where you get highly ritualized postures of different

types which have a most remarkable effect on the autonomic system. In the Indian physiological textbooks of a few centuries ago one finds all we know about the vagus nerve, for example, described in terms of kundalini, a great snake in the body; and very precise descriptions are given of how to control this section of the autonomic system, by tucking the heels up under the genitalia and so forth. These look like merely ritual gestures, but in fact are very crafty adaptations of the human frame for a certain cultural need of yoga or trance.

LORENZ:

I wouldn't say they were ritualized; they are dictated by the needs of the body.

FREMONT-SMITH:

Is it possible in the human that imprinting, if it occurs, is of a physiologically fundamental kind so overlaid with embroidery that we practically never see the pure form of the imprinting?

LORENZ:

I hardly know how to answer—of course, imprinting is always dependent on some I.R.M. being there, and Dr. Mead was quite right to say that there is a tremendous variation in this respect. That unhappily deprives you of the only possible character by which you could recognize imprinting and the I.R.M. at all without doing experiments. I agree with what Dr. Mead has already said of the innate components of human behaviour. They must be something occurring in very many cultures—that's about all you can say. As to experiments, I must ask you not to expect too much knowledge about imprinting in man from ethologists. There is one I.R.M. definitely found in man, that is the I.R.M. to snakes. This I.R.M. is present in about 50 per cent of people and in very many people it is already less selective than in some others. It might interest you to know that the horror of mice is due to the same I.R.M. as for the snake, because of the movement without legs. Snake movement has the legless character and also a winding motion, and there are people who do not mind the winding but do mind the legless movement, and those are the women who are scared of a mouse.

TANNER:

Could you quantitate the extent of that I.R.M. in an experimental way?

LORENZ:

Well, in human beings it appears you cannot quantitate it because you cannot quantitate the inhibitions of the higher central nervous components.

TANNER:

If you could it seems to me that the genetics of it could be studied.

LORENZ:

It begins to operate at about three years—in the three-year-old child you can nearly neglect the effect of self-control.

MEAD:

You have to have people who live in an environment where they know nothing about snakes, there are no words for snakes, no dances about snakes.

LORENZ:

These were children in Viennese hospitals who certainly lived in a world where they weren't taught anything about snakes.

MEAD:

Were there mice?

LORENZ:

There were no mice.

ZAZZO:

One can distinguish between, and to a certain extent compare, different cultural ritualisms and the codification of mimicry. One example which seems fairly clear to me is that of the different stages of smiling: the smile of appeasement after food which appears immediately in the infant and which gradually takes on significance and becomes a sign, an expression. It is fairly clear that we have here a tonic, postural origin of certain signs. Now, I should like to know to what extent this explanation, which is valid for smiling and certain kinds of behaviour and some mimicry, can be validated for the organization of certain cultural rites.

MEAD :

I am not quite sure that I understand your question. You have people who reply to the smile, mothers who spend hours smiling at their babies and playing with them. In other cases, the baby spends almost the entire time held on the mother's back in a bag, where it has all the cutaneous pleasure of being next to her. When it is hungry it can be fed, but it is much less face-to-face with the mother. The Balinese regard indiscriminate smiling as the first sign of insanity, and one of their definitions of an insane person is somebody who will smile at anybody and who will respond to a smile with a smile. The only adults who smile are the type we would probably classify as hebephrenic. The Balinese are the most unresponsive people in the world. They ignore oratory. If you try oratory on them they either go home or go to sleep, but you cannot make a speech to the Balinese and be heard. Probably this comes from their continuous stimulation of little babies. They smile at them and play with them, and they don't discriminate between smiling and tears in Bali. It is just as much fun to make a baby cry as to make it smile. They just want to make it do something. At first there is an exaggeration of the elicitation of smiling responses, until the baby is seven months old. The Balinese say 'as happy as a baby at its seven-months' birthday', and it is just at the height of its smilingness, responsiveness, gaiety. But by the time the child is about two-and-a-half, it has decided to quit. It ceases to respond. The adult finally does not respond when other people want him to do so. The Balinese smiles when he wants to smile, not when other people want him to, and grief is not permitted at all except on the stage, or in a mother of a baby dying under three months. If she cries a little then, they forgive her. Otherwise, no crying, no grief at all, though on the stage the representation of grief would be recognized by a European as beautiful deep grief.

LORENZ :

So you would see an expression of grief, perfectly intelligible to us, on the stage but not in real life?

MEAD :

Yes, on the stage you mourn when you are lonely, when you are deserted, when somebody dies, and it is a beautiful thing.

INTERVAL

LORENZ :

I would like to point out that in thumb-sucking, and in relation to psychoanalysis and the neuroses something very definite and

complicated comes in, which has not yet been discussed at all, and that is *displacement activities*.

HARGREAVES:

Would you give us a summary of what you mean by displacement activity?

LORENZ:

In certain situations, especially those of a conflict, or in other cases in which an activated drive is deprived of its normal outlet, there occurs a very striking phenomenon, called by TINBERGEN (1951) displacement activity. If an instinctive activity is released, activated by its normal I.R.M., and then another, conflicting, instinct is brought into play, which prevents the consummatory act from occurring, there happens something very surprising: the animal suddenly performs movements pertaining to neither of the two conflicting instincts, but to an entirely independent third one! When the fighting drive is activated in a stickleback and, at the same time, escape reactions are released, because the fish, though furious, is considerably afraid of his adversary, he will, after some vacillation between attack and retreat, suddenly start to dig in the sand, performing exactly the same instinctive movements as when building a nest. Two roosters, in the same conflict situation, begin to peck at the ground. Avocets do some displacement sleeping (the most amazing among all displacement activities), putting their bill behind the wing and, in this peaceful position, glaring furiously at each other with one eye. As you all know, humans, in conflict situations, start to scratch their head. In all displacements, it is usually a very common, primitive instinctive action which is released. Most frequent are so-called 'comfort activities', preening, scratching, yawning, etc.; perhaps you know the fits of displacement yawning in furious monkeys, especially baboons. Sucking certainly also is, among all mammals, a very archaic instinctive movement and it has been known to appear in displacement situations in apes, monkeys, and bears. I have no experience with thumb-sucking in children, but I am ready to take a bet that you will find it is a true displacement activity in moments of stress. In man and very highly organized mammals learned activities may also occur as displacement: women patting their hair, men adjusting their tie, both lighting cigarettes. Always these learned movements are highly automatized, kinaesthetically 'ground in'.

The term 'displacement activity' is, in my opinion, not a good one. It was chosen by people who did not know or care about what it means in psychoanalysis.

BOWLBY:

I want to emphasize that terminological difference; 'displacement activity' is where the tension belonging to one instinct is discharged by being short-circuited to another instinct activity. If we use this for the ethological concept, the term 'displacement' can continue in psychoanalytic usage to mean not hitting 'x' but hitting 'y' instead. In displacement it is the same instinct being discharged, but with a different object (see TINBERGEN, 1952).

LORENZ:

The first man to describe displacement activity was MAKKINK (1936). He talked about sparkling-over activities. In all conferences about terminology I still advocate the term 'sparkling-over activities' because that's exactly what they are.

I think much emphasis should be placed on the fact that there is first activation and then frustration. There is a little lack of clarity in Tinbergen's terminology because with him the word 'block' is used for the inhibition sitting on the automatism and removed by the I.R.M. But in the case of displacement activities he talks of a drive which is 'blocked', when it really ought to be 'reblocked'. An instinct must first be 'deblocked' and then 'reblocked' by a conflicting one.

MEAD:

If you have a person who when frustrated in love suddenly begins to play chess very hard, or a woman who after a love disappointment takes to eating on a large scale, would that be comparable to displacement activity?

LORENZ:

On a higher level, it may be, because if you ask me what analogy I see in psychoanalysis to displacement activities, I should say, I see it in two cases: in the neurotic symptom, and also, or at least something very like it, in sublimation.

HARGREAVES:

Do such things as displacement activity and imprinting in fact occur in mammals, and if so, under what circumstances?

LORENZ:

Well, to the question of mammals I have got a very simple answer. We don't know a thing about them, because of the purely technical

difficulty of rearing mammals from immediately after birth. We have tried to, we are working on it, but while for birds there is a tremendous tradition since the time of old Pastor Brehm, who taught us how to rear birds from the egg, there is no tradition on how to rear mammals. There is hardly even adequate knowledge of the milks of different mammals. There is no mammal which I should undertake to rear except big ruminants, let's say a sheep at least, and they take up a lot of room, and we haven't done it yet. We must keep in mind that an animal who is only just living is no use to us for our experiments. You remember that shrike who showed a serious diminishing of its I.R.M.s, when it did not get the right food. You can imagine what happens when you try to rear a dog or a sheep artificially: it would not be absolutely healthy. We haven't succeeded in rearing a newly-born rodent, except a guinea-pig, at all, and guinea-pigs are domestic animals domesticated by the Aztecs and show a tremendous variation, so that they are extremely inadequate for investigation of I.R.M.s. Then I must still further emphasize the immense difference in the sensory apparatus between most current mammals and man—a mammal which is so purely optico-acoustical as man is very hard to find, except for monkeys. Of course, if I had had chimpanzees, I would have done imprinting experiments with them, and then I could tell you a lot about the I.R.M.s and the resulting mechanisms. But, unless you take primates, which we intend to do when we have room and money, I cannot answer your question. Maybe in about five years I can just tell you something about small monkeys, or lemurs, with which we intend to start.

HARGREAVES :

What about the 'Mary had a little lamb' story? It is believed among English farmers that, if the lamb's mother dies, and the lamb is hand-reared, it afterwards becomes a complete pest to human beings. It's always about the house, it follows people, and this is the origin of the story of 'Mary had a little lamb, that followed her to school'. What effect does that have on the relationship of the lamb to sheep, both as a lamb and as an adult?

LORENZ :

Once you try to evaluate animal stories, connected with people who do not know what is important and what is not important, you find that they have absolutely no value. You just can use them as hints, and at present there is one lamb known, that was brought up not by Mary, but by Hediger, in Basle, that is definitely imprinted socially to man. The imprinting has been quite irreversible, but

unhappily the lamb is a female, and a female sheep is very passive, and there is no indication whom she would like as a mate, so there is no telling what man or dog or god-knows-what this sheep would like to copulate with.

FREMONT-SMITH:

I think Dr. H. S. Liddell had one lamb that was reared in the home, and I believe I am right in saying that this lamb did not run with the flock.

LORENZ:

Nor does that of Hediger. We must keep in mind one aspect of social animals. The most disagreeable thing you can imagine for a social animal is a closely related species. If you have greylag geese in a flock and a flock of bean geese flies by, they just look away. That is not innate, but certainly acquired. A new goose, not personally known to the others, is a pariah. A quite definitely negative social reaction is shown in an animal which belongs to one group towards the next group.

MEAD:

Do you attribute that to continual disappointment through false recognition?

LORENZ:

It might be. I might add something about the false recognition. You know what Seitz called 'Gespenstreaktion' in fishes—their reaction to ghosts—which we might call, in a less emotional way, the reaction of the broken-down Gestalt. If you change one essential character of this quality you change the whole complex and the whole Gestalt breaks down, rather as if one important chord or simple tone changed in a melody. This is very disagreeable. There are many instances where animals react to the change of single characters with escape reactions of extreme intensity. For instance, jackdaws are very afraid of a white jackdaw, an albino; so much so that you could not get such an intense reaction in a jackdaw from any other object of the same size. The same applies to fish presented with a formalin preparation of the same species. That's about the worst thing that can happen to them. Now, if you imagine that suddenly a man who is a formalin preparation walks in at the door, it would be very frightening indeed. He would have white eyeballs, a chalk-white skin and so on—a ghost, you see. Martina, my pet goose, who was tamed

to me as mother, and next to me to my wife, gave a fearful escape reaction when my wife sat in the canoe in my place. She didn't notice at once, and followed the canoe, when my wife pushed off. Suddenly the goose looked up, saw my wife, and incontinently dived, and came up yards away. That is a reaction of broken-down Gestalt, and something like that may be one of the reasons why the bean-goose may be a 'ghost' to a greylag goose. In favour of this explanation is also the fact that the animal can very easily get over its fright, and get habituated to, for instance, the white jackdaw.

GREY WALTER:

This is one of the features of animal behaviour that can be very beautifully demonstrated on a neurophysiological level. If one presents a human subject with a time Gestalt, that is a time pattern which is regularly repeated, it produces very complex responses in the so-called association regions which usually die away rather quickly, after a few seconds or minutes. Now, if you change one feature in the Gestalt, so that instead of making your flashes in one regular rhythm you change to another, then the whole picture explodes, and you see on the screen an electrical explosion as it were, which is far greater than that produced by any single item. Some people say that they feel 'swimmy' or faint or light-headed—they suddenly have this shuddering feeling very much as though they had seen a ghost.

HARGREAVES:

Is this mechanism related to the kind of horror one feels at the sight of anencephalic newborns and the rejection of them by the nurses who don't want to feed them, and the mother who wants one to die?

LORENZ:

Yes, I should certainly say so. But it might be that in that horror an I.R.M. also plays a role, because the anencephalics are just the opposite of the normal baby with its protruding forehead and typical proportions. Then I might remind you that all pictures of ghosts, and all devil masks, and even the Chinese dragons, are distortions of the human form; they are not animals at all, but they are men in a reptile skin with the head proportions of a man, a man with horns and a tongue.

I want to ask Dr. Grey Walter: do you find a correlation between the degree to which the Gestalt has become familiar and the intensity of the reaction to the breaking-down of the Gestalt?

GREY WALTER:

Yes, for some people, but that raises the enormous problem of types of personality. Some people show this, others show the reverse.

LORENZ:

Yes, the Gestalt perception is very different in different people, and it is to be expected that very different changes are necessary to 'break up' the recognizable quality. But this also depends largely on personal experience with the particular Gestalt in question, with its familiarity. Some animals, for instance my ravens, resent the slightest change in a very familiar environment, and do not mind it in a less familiar one. A new pile of wood in our courtyard would frighten them away for days, but the same pile on a forest glade two miles off did not matter to them at all; they sat on it.

Now, I have been asked to talk about innate releasing mechanisms in man. I am somewhat embarrassed, and in order to make my statement tolerably convincing, I must remind you of the difference between Gestalt perception and the summatory character of all I.R.M.s, of the independence of simple single key-stimuli. The only indication we have that something is based on an I.R.M. is that it will respond to single key-stimuli, in other words, that it will respond to dummies. That the acquired reactions generally do not react to dummies is a general rule to which we have hitherto found no exception. When we first experimented with a fish, *Astatotilapia*, we thought it to be pretty nearly impossible that the animals should possess acquired reactions to the other fish, because then all we knew about fish were Tinbergen's beautiful dummy experiments, in which he could do anything with both sexes with dummies. When we started to work with *Astatotilapia*, a fish with a very beautifully patterned and colourful male, and a cryptically coloured female, we could get all the reactions of a male to a dummy male but none of the reactions to a dummy female. The fish simply refused to react even to our most elaborate imitations of a female. Then my confidence broke down and I said: Well, here we have an I.R.M. responding to a Gestalt. Only I was lucky to have in Alfred Seitz a pupil believing more than I did myself, and he said 'Well, you must rear them from the egg'. We did so. Now, when one of these fish reached adulthood, Seitz, who was a very systematic man, said, 'Now we will start with the simplest possible model', and he rolled a ball of yellow plasticine, and stuck it on a glass rod, and slowly entered the aquarium with it. If I had been forced to take a bet then, I should have taken a bet to any sum that this fish wouldn't do anything at all, so convinced was I of my having been wrong about the I.R.M. and that the isolated

male would not react to the simple dummy any more than a normal male would. But what really happened was that this fish just looked at the plasticine ball, spread all its fins, trembled with emotion, and started in a frenzy of courting that ball. That was the most dramatic experiment I have ever witnessed. And the simplicity of the I.R.M. has been demonstrated again and again ever since.

FREMONT-SMITH :

Was it just the colour of the ball?

LORENZ :

No, it was just an object approaching and not flying away—one slowly approaching object. That's all the I.R.M. there is for the female in *Astatotilapia*.

MEAD :

Did you make beautiful models with all the patterns in them?

LORENZ :

At first, for the males normally reared, we had made models which we could hardly tell ourselves from real females, they were so beautiful, and we got no reaction whatever. That was with cichlids. And cichlids are, of course, much more intelligent than sticklebacks—they have a much higher level of Gestalt perception.

This preamble is all to say that, where we get reactions to dummies, we have a very good reason to suspect that there is an I.R.M. Now, in man, there are quite a number of reactions which definitely will be elicited by extremely simple dummies, and among these are all the reactions of man to the expression movements of the fellow-members of his species. Everybody knows by self-observation that if we use animal heads as dummies, and observe people in zoos, we find that morphological characters of an animal's head are invariably interpreted as if they were movements or postures of the human head and face in expressing some emotion. If the eagle is the symbol of strength and courage—actually he is less courageous than a raven—it is all because the eagle has, by virtue of the bone-covering of its eyebrows and the form and the angle of its mouth, the big eye looking foward, something of what we call the 'hero face' of the human fighting male. We cannot get away from that, and the emotional value of the eagle face is inescapable. Just as little can you escape the stupid superficiality of the head of the camel or the llama, and that is only because the head is permanently held slightly above the horizontal, so that the nostrils are higher than the eye, because the lid of the eye

comes slightly down, as a protection against the desert sun, and the nostrils are narrow slits as a protection against the desert sand. These are all morphological characters, nevertheless you cannot prevent yourself from interpreting them as human expression movements and from feeling that the camel is very stupid and supercilious. You can go on giving examples for hours and hours, for instance, in the mandarin duck, the upward curve from the corner of the eye makes you feel that it is smiling, while in the closely allied North American wood duck, the female has a white patch under the eye and that has got something of a distressed look, as if it had wept. So, we all have a lot of these reactions to dummies represented by the heads of animals. But you can go down to simplifications which go way beyond the simplification of human expression proportions. When I was a child a certain type of railway carriage in Austria, which had narrow and high windows and had a ventilation slit just above the window, had to me a very disagreeable, frightened look because, quite understandably, these slits were interpreted as eyebrows, and the face was a long face; while the Pullman cars, which had broad windows, and a sort of line beneath, looked large and happy. HEINZ WERNER (1933), a Gestalt psychologist, thought that this kind of experience, of dynamifying experience of environment, was a genuinely primitive character of all experience as such. In my opinion, all the dynamic experiencing of environment is due to a miscarriage of an I.R.M. directed to the expression of emotion in man. We must always remember that one key-stimulus alone in the supranormal object may elicit a reaction that is qualitatively the same as the whole set of key-stimuli. This simplification may go on so far that something like the proportions are reacted to as being beautiful, even if they are realized in the most exaggerated way. Our own reaction to such supranormal proportions is shown by any fashion paper.

Something interesting about all these I.R.M.s is that all domestication characters elicit a negative emotional reaction. When the artist, from Greek sculpture onwards, tries to present something ugly, he does not represent any old distortion of the human form, but a quite definite distortion of being too short and fat and bow-legged. On the other hand, the artist can exaggerate the opposite proportions to any extent, he can paint a man with shoulders broad and legs elongated to any extent, and these exaggerations are taken in without any resistance of our sense of beauty.

MEAD :

I want to be sure that I have got this last point. Do I understand that the characters that are used to produce something unpleasant

will be ones that are characteristic of domestication in animals?
Short and fat?

LORENZ:

Yes, shortness of legs, bow legs, loss of muscular tonus, sagging belly, small and lustreless eyes, etc., etc. In a series of photographs representing domestic animals in comparison with their wild ancestors, it is quite amazing how extremely ugly the domestic forms appear, and they do so to everybody, not only to biologists.

But now let me proceed to what interests us most, the mother-child relationship. One of the best instances of the I.R.M., except for the snake, is our reaction to the quality of *cute*. In literary German there is no word for this, but in the Austrian and Bavarian language there is the word *herzig* which implies the verb *herzen*, to fondle. The word which most succinctly means just this and nothing else is the American slang word *cute*. Now, let's look at the properties which produce the impression of a thing being *cute*. The head must have a large neurocranium and a considerable recession of the viscerocranum, it must have an eye which is below the middle of the whole profile. Beneath the eye there must be a fat cheek. The extremities must be short and broad. The consistency of the body ought to be that of a half-inflated football, elastic; movements that are rather clumsy elicit the reaction very strongly, and finally the whole thing must be small, and must be the miniature of something. Now, if you observe yourself and your reactions to different animals, you will find that wherever one of these qualities is present, you react with a feeling of that particular kind. In the German language, where there are many diminutives, this expresses itself in the names given to animals. All animals whose German name ends in 'chen' have at least these head proportions. If something is a miniature of something very big—an elephant, but you must know how big an elephant ought to be—then you find how sweet the baby is. You must know how long the trunk ought to be in order to interpret the trunk of the baby elephant as being much too short for an elephant. I want to stress the relational property of those key stimuli. You may even make a miniature of a pipe, and you say, 'oh, how cute'. The child is miniature. Another heterogeneous summation can be demonstrated very nicely in animals which, for instance, have not short legs, but long legs, as, for instance, foals or lambs. Nevertheless, you find that the degree of cuteness is always dependent on the number of key stimuli realized in the object. The cheek is very important and the *corpus adiposum buccae* is also very important, and its lack in monkeys does much to detract from the cuteness of little monkeys. Also a very important part is the tactile stimulus—the rounded behind of

the baby, because you feel that when you carry it, and monkeys who haven't got that are distinctly repulsive. If you see chimps and young gorillas side by side (the gorilla is a ground animal and has a large gluteus maximus where it ought to), it is infinitely sweeter than the chimp who hasn't got it. Now, in order to see whether many people have got that I.R.M., we ought to do a mass experiment with thousands or millions of experimental persons. Just this experiment has already been done: it has been done by the doll industry, which, of course, sells the supranormal object best. The exaggeration of key-stimuli can be very nicely shown in the 'cupie' doll, and the 'Käthe Kruse Puppe' in German, and if you want facts on what I say, then go to Walt Disney's films and see how Walt Disney represents cute animals.

I want to say one word more on the *corpus adiposum buccae*. Many speculations have been written on its function. Some have said that it helps in sucking. Now I don't see why the monkeys, who have got a much longer snout, are able to do without it. I propose the theory that the *corpus adiposum buccae* is really a releaser. It is there for the very purpose of eliciting an I.R.M. in the adult. That is not so speculative as you may think, because quite a lot of the releasers, which have definite correlates in the male's I.R.M. to the female, are nothing but fat—fat determining body outline, in breast, hips, and so on. So that it wouldn't be so very surprising if the *corpus adiposum buccae* should be the same, especially if there is colour on it.

Now, a dig at psychoanalysts. I agree with McDougall that emotion is the subjective side of an instinct, of one particular instinct. One qualitatively isolatable emotion is subjective to one instinct. Now, I feel that I can in myself very clearly differentiate between my reactions to the key-stimuli emanating from the female and my I.R.M. reactions to the baby. I think that I can, by introspection, assert with a quite considerable degree of certainty that what I feel while fondling a lion cub, a chow puppy and a baby is qualitatively the same, and different from what I feel when sexual I.R.M.s are brought into play. I would even go one step further and assert—I would perhaps not dare to publish it, but I do dare to say it among friends—that I can introspectively recognize the workings of an I.R.M. as such. It gives you quite a particular feeling, I should say the ego is always surprised at the unexpected and independent reaction of the id—I don't know whether I quite succeed in expressing what I mean. But anyhow—what I want to emphasize is the value of introspection, of psychology in the strictest sense of the word: it can tell the objective behaviour student quite a lot about himself, and he ought to be interested in that animal, too.

Now I want to proceed to a rather passionate theme. It concerns

one of the very few pure instinctive movements of man, a movement of expression. We have already spoken about our own reaction to it, of our reaction to the 'eagle-' or 'hero-' physiognomy. When you are brought into a situation in which the fighting male in you 'believes' that it must go in defence of something, then you get, as reaction, an expression movement involving the whole human body and one of the few real instinctive activities of our species. If you are an enthusiast this reaction will occur whenever you are stepping in for an idea, for something that ought to be defended, it may be your nation, it may be the old school, it may be the freedom of scientific investigation. In all these cases you behave in a very singular manner. You feel a prickle going down your back, 'ein heiliger Schauer'—and it is quite characteristic of the German language that this is holy only in German—and I am not making fun at all. Then the tonus of your musculature goes up. Your arms are slightly abducted and go forward, you make the hero face, and then you are ready to do anything, to lose yourself, to forget yourself, in the good and the bad sense of the word. You are selfless and ready to die for the society, for the super-ego which you are about to defend, and you are also ready to do something absolutely foolish. Now, I affirm that a man who has not got that back-prickling reaction is an emotional cripple, and I wouldn't like to have him for my friend. But a man who has got that reaction and doesn't know about it is a danger to humanity, because what everyone ought to know about this reaction is this: when a chimp is aroused to enthusiasm, that is to say in defence of his family, he thrusts forward the jaw, he throws outwards his arms, and he fluffs out his pelt. It is very illustrative of the extreme conservatism of instinctive reaction that in this reaction we not only fluff out a pelt but we still throw out our arms in such a way that the pelt will stick out in a direction in which it serves to make our outline bigger and more imposing when we are facing or expecting our adversary. I think that this dilemma is very characteristic of the whole of humanity and of humanity's need to know and govern its own instincts. If there are no instincts, if all instincts drop out, then you get an emotionless, feelingless model man of the *Brave New World*. If you have this reaction, but don't know about it, then you are the victim of any demagogue.

TANNER:

These matters of the evaluation of and reaction to body images have been very much on my mind for some years. There is a vast literature about the way in which animal heads and animal expressions have been used by the human to signify certain things. It was most prominent about the time of the Renaissance, the best-known work

being by DELLA PORTA (1668); this is a very famous series of drawings in which different people with different aptitudes were portrayed with different animal heads and expressions. This literature also goes down to the detail of individual expressive movements, particularly in the Indian dance literature; the various gestures in the Indian dance are often referred to by animal names. It seems to me that the interpretation Dr. Lorenz put on this may very well be the correct one. I say advisedly 'may' because I do not think it is certain that these things are I.R.M.s rather than learned.

Dr. Lorenz mentioned the idea that to make a person's image into a devil's image, you change his shape by spreading him out in a particular way. I do not believe that you only do that; I think you use caricature. I believe this is a matter of the valuation of a particular physique. I have hanging in my office a very amusing cartoon by Vicky of two English politicians whom most of you will know, Cripps and Bevin, both now dead. It happened at the time this cartoon was done that a British ministry put on an exhibition to popularize certain ideas, mainly the Marshall Aid Plan for Europe, and in this exhibition they had distorting mirrors which made you fat and thin. Vicky had drawn Bevin, who was massive and nearly spherical, looking at his long-drawn-out thin image in one mirror, and Cripps, who was very slender, looking at a totally inflated image of himself in the other. Under the first mirror was written 'without aid' and under the other 'with aid'. As an illustration of D'ARCY THOMPSON's (1942) famous method of transformed co-ordinates this was amusing, but it was also instructive, I think, of a great deal more. There are barriers between people which are to do with their different physiques but are not due to I.R.M.s—rather to the valuations we put upon physique and our identification positively or negatively with different builds. People built, for example, like Bernard Shaw, are simply not regarded as human by certain others, and equally those of the Bevin build are regarded as sub-human by some of the other sort of people. One must be continually aware of the difficulties of communication across these barriers, but they may not be difficulties of an instinctual nature. If they were the devil-distortion might be expected to go in one direction only. But I doubt whether it does this and I want to ask Dr. Mead what she thinks about that in other societies. Are there places where the long, thin ones are despised, and made the prototype of the devil?

MEAD :

I would be prepared to believe that there are certain sorts of distortion in either direction which, if they were too great, would be unattractive.

LORENZ:

I saw once in the *National Geographic Magazine* a picture of a black chief with a row of about seven wives standing behind him, in the order of their favouritism. They were quite exactly in the order of their hour-glass form.

MEAD:

That would be completely reversed in Bali. The Balinese make their monstrous figures of the witch out of the most masculine and the most feminine characters you could think of. The witch has big breasts—the Balinese think they are loathsome—and she has hairy arms as well.

LORENZ:

The favourites in the picture had little breasts.

MEAD:

Yes, but there are plenty of societies where that is not true, where big breasts are the important thing, and where the tiny breast would be rejected. The Balinese reject both extreme male and extreme female characters and prefer the male and female that are—well, GEOFFREY GORER (1936) once made a crack about it and said you could not tell a Balinese male and female apart, even from the front. This is almost true. If they are clothed to the waist you cannot tell the male from the female, say at about nineteen or twenty years old, from a short distance. They devalue curves, breasts of any size or any degree of pendulousness, and hairiness or muscles on the male. They are quite capable of developing muscles, and if you turn a Balinese into a stevedore he develops perfectly good ones. But their way of life is such that they have almost no muscular development. So that you have to postulate a complex mechanism, part of which might be an I.R.M. and part of which is imprinting or learning the type which is approved in your own society—whether that type is simply the type you see in your parents, highly valued or devalued.

LORENZ:

But don't these Balinese girls have beautiful female sexual proportions?

MEAD:

They are very under-developed as females. They have very small, very high breasts. The standard Balinese figure is such that you carry

the baby high, and the baby drinks from the upturned breast. In some cases you get a pendulous breast, but the mother still carries her baby up high. To say that that is a beautiful female figure is a matter of the aesthetics of any given people.

LORENZ:

Does any people prefer very big pendulous breasts?

MEAD:

Yes, there are societies in which the Balinese woman would not get married.

May I return to the point about domestication; you said that characters producing the feeling of unpleasantness are those associated with domestication. That seems to me to lay overmuch emphasis on biological factors.

LORENZ:

This is, of course, quite true, and I am quite ready to accept that this is a simplification. About domestication, it seems to me—and I refer to our explicit permission to talk suspicions—that the same holds true to a certain extent for the behaviour traits of domestication. You hardly find one domestic animal in which sexual behaviour and eating behaviour is not increased quantitatively in an enormous degree, and the more complicated forms of social behaviour, caring for young, and so on, decreased in intensity. This is so general among domestic animals that I would find it very hard to find one instance of a domestic animal that is more social than the wild one, except, of course, the dog. Now, I think that in our emotional evaluation of our own instincts we put a very great plus value on those things which in domestication tend to atrophy and to disappear, and a minus sign to those which tend to hypertrophy. Everything we call bestial and brutish is not characteristic of animals in general but quite exclusively characteristic of domestic animals. I mean, if some wild goat were to talk about the proverbial lecherousness of the domestic buck, he would say, 'Poor fellow, he has been under human influence for centuries.' I see this daily in greylag geese as compared to domestic geese. You get similarities to human beings which are surprising, and you cannot help yourself from having sympathy with those nice, restrained, non-bestial wild ones and feeling some contempt for the domesticated ones. We must keep in mind that mother-love is not more necessary to the survival of the species than the drive to copulation. Why, then, are those drives to copulation 'brutish' and why is 'maternal love' sublime? This is simply our

emotional valuation of instinctive behaviour in man—and it is largely dependent on supply and demand. I am convinced that we have something very deep, innate, in our behaviour, which tends to devalue sex and eating and to value very highly mother-love, social behaviour, defence of family, and so on.

MEAD :

You can find societies which put a high value on sex and eating, and a low value on maternity, and it would be important to make a list of these atrophying and hypertrophying characteristics, and then see how they are combined in different societies. For instance, in the Marquesas, which is about the most pathological society that we know much about, women dislike having children very much and maternity, breast-feeding and care of the child are devalued. Women are reduced to 50 per cent of the men, so that every woman can have at least two husbands, whom she keeps by her sexual attraction, and maternity destroys the woman as a sexual object. Food was highly valued and cannibalism highly institutionalized. This was one of the most vulnerable societies we know anything about; it almost disappeared with European contact, in spite of a big population and a good food supply. Comparison of these points in different societies has enormous possibilities if we bear in mind that societies can organize these things in different ways.

LORENZ :

Let me ask a very special question of Dr. Mead about the Marquesas. We quite agreed on the fact that there is a supranormal object and that this supranormal object may cause imprinting in a non-desirable direction. We must not forget that in imprinting we get all the accidental characters of the object which causes imprinting linked up irreversibly with the releasing key-stimuli of the object itself. Do you think you can get sexual imprinting of young males to an unbiological cover-girl—in your 'pathological' Marquesas sense?

MEAD :

Well, Bali is on the edge of it certainly. To have a whole society in which the bulk of young males is imprinted to an inaccessible object in some way—I do not know of any extreme behaviour like that. Bali is a case where you fall in love with your not-mother, or, taking it the other way, perhaps, the attitude toward the mother is sexually inhibiting to a very marked degree. In all theatricals, the hero tries to get a beautiful girl who is not like his mother, and the tragedy is

that you are always trying to marry the slender, far-away, not-mother figure, and you end up married to a Balinese woman. There is a very high degree of avoidance between husband and wife. The marriage ceremonies are filled with highly ritualized jokes on the possibility that the husband may never consummate his marriage at all and never have any children, in a society which is organized to make people have children.

This is not as simple as a simple imprint at all. You have simultaneously presented to the young child the witch, embodying the fear of its mother, and the beautiful princess he would like to have but whom he can never get. This is represented over and over again in a whole series of forms, so that the child is getting treated by his mother in a witch-like fashion and sees the supranormal witch played by a very tall man with all the exaggerated characteristics, and at the same time sees his other visionary type of sexual object. The Balinese define the delights of sex as starting with the first interchange of glances and going steadily down.

LORENZ:

Now, just one thing. I want to get your reaction to this. If you compare—I hope you will forgive my using a goose and not a mammal—a wild goose, you know that the first sign of her love is just a glance at him, and when he displays, apparently she looks everywhere else except at him, but really she does look at him, but so quickly that nobody notices it except him: at least, if she doesn't look at all, he ceases the display immediately. From these first preliminaries the whole thing very gradually works up to neck dipping, to the triumph ceremony, and so on, until at last she invites copulation. Now, a pure wild greylag goose is absolutely unable to come to copulation in any other way except beginning with a glance and working up through the whole sequence of those reactions. The copulation of the domestic goose is totally independent of all these preliminaries, and when she wants to copulate she obtrudes herself to a male with three or four neck-dipping movements and then invites copulation, and that is all. Yet she differs from the wild one only in that the activities of lower intensity are skipped over or dropped out and the action of highest intensity appears without these preliminaries; the whole step-ladder of intensities leading up to it in the wild bird has become unnecessary. Now I think that I see this in human beings. There are some that will start with very hot glances, and love is likely to be immediate. The activities are the same which occur in a 'normal' woman at a very much later stage and the difference is only that the preliminaries have dropped out.

Now the question is, have you got something like these differences—of a very long ladder to copulation, or the dropping out—in your different cultures?

MEAD :

You have whole cultures that represent either type. For instance, among American Indians courtship might take four or five years. The girl never speaks to the boy, but there would be a slight exchange of glances, maybe for two years. Then at corn-husking, if she had got to the point where she wants him to do something really rather desperate—to speak to her—she might try to get from the ears of corn a pure red ear and then carry in her arms a bunch of corn, one ear of which was the red one, and as the boy went by he might say, 'That is a red ear of corn.' That would do for the next year.

Then perhaps for another year or so he would go hunting with her brother. Then when you finally get marriage you go through a very formal presentation, sending game home by her brother to her father and so on. These Indians are one of the few people we know that have a long period after marriage before copulation, and in many of these societies the girls wear a sort of chastity belt for quite a long time after marriage. Husband and wife lie awake and talk. You have enormous romanticism about the husband talking to his young bride—he has been wanting to talk to her for seven years and has not had a chance! This occurs in an area also where we have the institution of the copulation blanket, a special leather blanket which it is necessary to borrow from a chief, in order to beget a child legally and ceremonially, so that it will be the right kind of child, and where people boasted of not having children within five years of each other, or seven years of each other. It is pattern of extreme restraint and extreme romanticism combined. Then you can have societies with all the other points in which they behave just like your domestic geese. But I see one other even more important thing in what you said, and that is that very rapid culture mixture and very rapid social change have the same effect on human beings in vulgarizing the responses and destroying sequences that domestication has on animals.

LORENZ :

It is always a matter of breaking up or dissociation.

MEAD :

Of these elaborate patterns: the training of individuals in this excessively elaborate series of preliminary requirements before a mate could be chosen.

TANNER :

To come back to what Dr. Lorenz was saying; you could, of course, speed up the response considerably by giving a sex hormone. This is true of most mammalian species.

LORENZ :

There are two processes working in the same direction. The loss of selectivity of the I.R.M.s in the domestic goose is accompanied by a tremendous increase in hormones, which is quite clearly indicated by the fact that the domestic goose lays eggs in the first year and the greylag goose only starts laying properly in the third. On the other hand, you get considerable independence of the ceremonies from the hormones; because all these rituals do develop in the domestic goose much later in life. She starts by being absolutely promiscuous, because she is sexually mature before all her 'marriage' ceremonies are mature, and these are what keep the pair together, and make for monogamy. So that in domestic geese who do develop a 'triumph ceremony'—not all of them do—you have a stage of absolutely promiscuous life ending up in the slow development of the triumph ceremony, which matures later than normally and in the end there results a monogamous couple quite similar to the wild ones.

TANNER :

In general, I suppose, hormones don't affect the sequence of events, but they lower the threshold.

LORENZ :

Yes, they lower the threshold. They may have a big effect and they may have no effect at all, and that there is no effect on behaviour sequence can be even shown by the potential homosexuality of some birds. When you haven't got a male, the female may do with another female all the things that a male normally does, up to copulating, but while she is behaving absolutely like a male, she lays eggs, which shows that her female hormones are quite all right. We thought for a very long time that in cichlids sexual behaviour was dependent on a quite definite hormonal stage which A. A. ALLAN (1934) thought too when he wrote his paper about 'synchronization of the mating cycles'. We were quite sure that this was the case with all fish, until this year when Beatrice Oehlert found a bigamist fish who had a wife and children which had just hatched out of the eggs. He knocked down and chased away the male of another couple at the same time in another corner of the same tank and occupied his territory and his

wife. Then he went to and fro and behaved in quite a different manner with his new wife and his old wife, doing nest-building and inseminating activities in one corner, going back and doing the phases of three weeks later with his first children, then going back and fanning the eggs with his second wife, and so on. This showed that he could change his reactions within seconds, quite independently of his hormonal state.

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